POLITICAL SCIENCE QUARTERLY

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Edited for the Academy of Political Science by the Faculty of Political Science of Columbia University

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Published quarterly at 111 East Chestnut Street, Lancaster, Pa., for the Academy of Political Science, Fayerweather Hall, Columbia University, New York 27, N. Y. Entered as second-class matter, March 14, 1911, at the Post-Office at Lancaster, Pa., under the Act of March 3, 1879. Second-class postage paid at Lancaster, Pa., and additional offices.

Library of Congress Catalog Card Number: 7-36315.

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Volume LXXIX *March 1964* *Number 1*

POLITICAL SCIENCE QUARTERLY

THE UNITED STATES INTERNATIONAL-PAYMENTS DEFICIT: DILEMMAS AND SOLUTIONS *

I

IN RECENT years very substantial advances have been made in the general theory of international-payments adjustments, especially with respect to the effects of changes in the foreign exchange rates. But it is often difficult to pass directly from the broad conclusions which these various analyses suggest to the formulation of specific policies that are likely to prove both relevant and acceptable in particular concrete situations. The difficulties may arise from either of two main sources. One is the fact that nations are usually trying to achieve a number of different economic and political objectives simultaneously, even though these objectives may not all really be mutually consistent. The second is the fact that both the theoretical analyses and the practical conclusions which can be drawn from them sometimes require assumptions that may be unrealistic in specific real-world situations, and that may hence impair or even completely destroy the applicability of the analyses and conclusions themselves.

The problems raised by the recent position of the balance of international payments of the United States, and by the remedies that have thus far been tried or proposed, provide striking illustrations of some of these difficulties.

The dangers in the U.S. payments position are familiar to

* The present paper is an extensive revision and expansion of my article, printed in Italian with an equivalent title, published in the *Rivista Internazionale di Scienze Economiche e Commerciali* for January 1964. I am indebted to Professor Tullio Bagiotti, editor of the *Rivista*, for permission to publish this revision.

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almost every newspaper reader. Ever since 1958 the United States has been running large international deficits, which have averaged some $3.5 billion a year and which in the second quarter of 1963 rose to an annual equivalent rate of over $5 billion.[1] These deficits have been met by gold sales of some $7.2 billion (end of 1957 to October 1963) and by increases in U.S. short-term liabilities to foreigners of roughly $10 billion. The U.S. monetary gold stock has fallen to under $16 billion, while total liquid liabilities to foreigners —which are or can become direct claims on gold—have risen to over $26 billion.

It is obvious that the United States cannot allow these huge annual deficits to continue indefinitely. If they were all settled in gold, at the recent rates they would exhaust the present excess of our gold stock over legal minimum reserves in little over a year, and would exhaust the entire U.S. gold stock itself in perhaps four to five years. Moreover, at any time before that a new international crisis of confidence could suddenly erupt, as it did in 1960–61, with both foreign creditors and Americans themselves fleeing from the dollar. The resulting violent pressures on our foreign exchanges could quite conceivably sweep away the new dollar defenses provided in the last two years or so by the so-called Vienna arrangements, the currency swaps and standbys (discussed below) set up by the United States with foreign monetary authorities and with the International Monetary Fund, and by other measures, and could even consume our remaining excess gold reserves. The United States would then be compelled either to let the gold stock be drawn down below the legal minimum, to impose comprehensive exchange controls, or to devalue the dollar—and perhaps all three—and presumably to suspend gold payments. Whatever the action taken, the resulting shock and damage throughout the non-communist world would be severe. It is clearly to every "Western" country's advantage to help prevent such an outcome.

But while everyone concurs in these last propositions, it is less easy to achieve agreement on the countervailing steps

[1] The deficit for the third quarter of 1963, the most recent period for which data were available at the time of writing, had dropped to an annual equivalent of only $1.5 billion, but much of this improvement was apparently due to non-recurrent "financial" factors, especially advance repayments of previous loans abroad by the U.S. government.

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which should be taken. The difficulty is not merely that different men have different opinions, but also reflects the fact that serious dilemmas are inherent in the situation of the United States itself. Two are especially important. The first arises from the relatively low rate of U.S. economic growth in most recent years and the persistence of high unemployment. The *rate* of growth in the gross national product as measured in real terms has been slowly falling, on the average, since perhaps 1951, and despite the recent spurt in output has not averaged much over two per cent in the last few years.[2] When measured per capita, the average rate of growth in real terms has been less than one per cent, while unemployment has run at rates of 5½ per cent or more for years. A major objective of the U.S. administration has hence been to increase the growth rate substantially and thereby also to reduce unemployment. But at least since Keynes, the standard remedy for unemployment and retarded growth has usually been fiscal expansion, deficits if necessary, and monetary ease—all policies likely to aggravate rather than to end international-payments deficits. On the other hand, the classical cure for international-payments deficits is monetary restraint and even contraction, which is of course precisely the *wrong* policy for stimulating growth and employment. Other conflicts among our objectives and policies will be pointed out later.

The second dilemma arises out of the nature and sources of the U.S. international-payments deficits themselves. The deficits have not originated in the merchandise balance of trade, which on the contrary has usually shown substantial though varying surpluses. Rather, the main deficit items have been the very large volume of U.S. private capital exports (over $3 billion annually in most years after 1955), direct military expenditures abroad (also roughly $3 billion a year), and that portion of the U.S. foreign economic assistance programs which is disbursed abroad (now perhaps twenty-five to twenty-eight per cent of the total,[3] though a good deal higher

[2] See U.S. Department of Commerce, *Survey of Current Business* (Washington, D. C.), *passim;* and U.S. Department of Commerce, *Statistical Abstract of the United States for 1963* (Washington, D. C., 1963), especially the table on page 323.

[3] See Walter S. Salant *et al., The U.S. Balance of Payments in 1968* (Brookings Institution, printed by the Joint Economic Committee of the U.S. Con-

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in the past). But neither the second nor the third of these debit items could be substantially reduced by domestic monetary restraint as such; and if the restraint subsequently led to falling domestic profit prospects, private equity investment abroad might actually be stimulated. Exchange depreciation, another possible solution for international deficits, would also have little effect on the foreign currency volumes of the second and third items, and indeed might even make the general pressure on our balance of payments worse if further depreciation were feared. These problems, too, will be amplified later.

II

To deal with these and other dilemmas—to reduce or end the international deficits without incurring one or another major cost—several measures have been tried, and a variety of others have been proposed.

In the first group, of measures already adopted,[4] the one with perhaps the greatest long-run promise is the attempt by the U.S. government to induce the other principal industrialized countries of the non-communist world to take over much larger shares of the combined financial burdens of the common defense and of economic aid to underdeveloped countries. But this endeavor is requiring long and difficult negotiations. Persuading the other countries that their long-run gains will outweigh the short-run sacrifices entailed is not easy, and perhaps the old myth of limitless American wealth still persists. Meanwhile, for obvious reasons of high international policy, the United States must necessarily try to keep the programs going. Continued efforts to increase the proportion of funds which is spent in the United States, however, under both the economic assistance and the military programs, are reducing somewhat the resulting pressures on our balance of payments.

Second, efforts of various kinds are also being made to increase the international competitiveness of American industry, especially by increasing productive efficiency, im-

gress, [Washington, D. C., 1963]), 160. See also the Joint Economic Committee's own study, *The U.S. Balance of Payments—Perspectives and Policies* (Washington, D. C., 1963), 27.

4 For a detailed and useful summary of these measures to date, see the Joint Economic Committee study just cited, 35-38.

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proving market research and selling methods, and expanding governmental export credit facilities and guarantees. But the leading rivals of the United States are naturally engaged in closely similar efforts. It is true that in Western Europe the great booms of the nineteen-fifties have given way to rising costs, but the resulting pressures on profits have intensified rather than reduced Europe's continued drive for export markets, and in some fields have apparently led to selling abroad below true costs and to still greater lengthening of credit terms. In Japan, the rapid pace of general industrial progress and the broadening of international competitive ability likewise show no sign of flagging. The Common Market also threatens to become increasingly protective against outsiders. It therefore seems unlikely that any very dramatic improvement in the American balance of merchandise trade will result from our trade-promotion efforts. Indeed, we may prove to be doing rather well if we merely hold our present overall position.[5]

Third, early in 1963 the U.S. administration proposed a program of tax reductions, and at roughly the same time began an attempt at differential interest-rate manipulation. The tax proposal was, of course, designed chiefly to stimulate internal economic growth. It was accompanied, however, by the assertion that the reduction, by making domestic profit prospects more favorable, would also increase the attractiveness of investment in the United States to both foreign and domestic capital—thus producing a doubly favorable effect on the U.S. balance of payments. How large this latter effect would be, however, is again not clear. The effect on our foreign investment in petroleum, which is the largest single category, would presumably be negligible; and an appreciable fraction of the remainder of our foreign investment probably arises from the desire to get inside the tariffs and other trade restrictions set up by other countries. The interest-rate operation, on the other hand, has had an unambiguous effect on the balance of payments. This operation,

[5] The most recent attempt to forecast U.S. exports seems to me a good deal too optimistic, especially on prospective changes in U.S. productive efficiency relative to Europe and Japan and on increases in trade restrictions abroad. See Salant *et al.*, Chaps. 3, 4, 8, and 9; and my comments on this forecast in the Joint Economic Committee's *The U.S. Balance of Payments: Statements* (Washington, D. C., 193), 26–33.

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popularly called the "twist," was an attempt to raise short rates, thus helping the balance of payments by keeping U.S. short-term capital at home and even attracting foreign funds, while at the same time leaving long rates unchanged and thus avoiding any injury to economic growth. The operation was clearly successful in the sense that while rates on Treasury bills and other short-term paper were forced up sharply after May 1963 (to mid-December, by about sixty basis points for three-month bills), rates on long-term governments rose very little (by roughly ten to twelve basis points); and there was undoubtedly a substantial effect on international short-term capital movements—an effect reinforced by the special tax discussed in the next paragraph. This new interest-rate program does mean, however, that at least for the time being the policy of trying to stimulate internal economic growth by affirmative monetary means has been abandoned; [6] and indeed it is hard to believe that the maintenance of substantially higher interest rates, even at short term alone, can avoid having an eventually adverse effect on the rate of internal growth.

Fourth, in the summer of 1963 the U.S. administration proposed a so-called interest-equalization tax on the purchase of foreign securities. Depending on the character and term of the securities, the tax could run as high as fifteen per cent of the market value, which would reduce the effective yield of a seven per cent bond by roughly one per cent. The proposed tax, which if passed by the Congress will be retroactive to July 18, 1963, was designed to compensate for the higher rates of interest which had been prevailing in leading foreign centers, and thus to reduce the outflow of private American capital. The occasion for the proposal was the tremendous increase in actual and proposed security flotations in New York in the first half of 1963. The initial effect was spectacular. All new foreign flotations, and nearly all other U.S. purchases of foreign securities, were brought to an abrupt halt; and for this and other reasons the U.S. balance

[6] Total Federal Reserve Bank credit outstanding has fluctuated around a virtually constant average level since the end of June 1963; and in July 1963 Federal Reserve Bank discount rates were raised to 3½ per cent (previously three per cent). Also, while time deposits at commercial banks have continued to grow rapidly, demand deposits have risen very little on the average since the end of 1962. Member-bank reserves have remained virtually unchanged since late 1961.

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of payments deficit for the third quarter of 1963 dropped to a figure only thirty per cent as large as that for the second quarter. But the probable longer-run effect of the tax in improving the U.S. balance of payments is less clear. If it is enacted into law, many Canadian issues and certain other securities will be completely exempt from the tax, as will all investment in "less developed" countries, and also all other direct investments wherever located which give the investor more than ten per cent of the voting stock of the foreign enterprise concerned. Yet in 1960 through 1962, our "direct" investment abroad averaged roughly $1,600 million a year, or some sixty per cent of our total long-term private capital exports. And a number of other loopholes and leaks may also develop—for example, through short-term capital exports or the manipulation of merchandise transactions. Finally, since the proposed measure would merely be a tax, not an outright prohibition, nothing would prevent an insistent prospective foreign borrower here from offering a yield higher than what would otherwise be necessary by just enough to offset and thus absorb the tax. In the light of these various considerations, the proposed tax can hence be criticized as being both seriously and in part irrationally discriminatory, and of seriously limited probable effectiveness.[7]

Finally, beginning in March 1962 the Federal Reserve system and the Treasury embarked on a remarkably imaginative and varied set of measures, referred to briefly above, which were designed to increase greatly the international liquidity of the United States as well as of other countries, and thus to build up additional defenses for the U.S. dollar.[8] Of these measures the most novel was the establishment of a series of reciprocal currency agreements or "swaps" with

[7] Thus exempting all large "direct" investments seems a discrimination in favor of large U.S. international enterprises at the expense of, among others, the small U.S. would-be investor abroad, who can only invest by buying securities. In 1960–62, purchases of portfolio securities averaged some forty per cent of total private long-term capital exports by the U.S. Under the proposed new legislation, most purchases of this type would be taxed. Some observers have also described the proposed tax as being in effect a "tariff on capital," and recommend instead a capital-issues committee (a device which is also proposed, in substance, in Section V, below). See *The New York Times* editorial pages for December 9 and 13, 1963.

[8] See the *Federal Reserve Bulletin*, XLVIII (1962), 1138–53 and XLIX (1963), 311–21 and 1216–23; also "Conversations on International Finance," the Federal Reserve Bank of New York *Monthly Review*, XLV (1963), 114–20.

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ten leading foreign central banks and with the Bank for International Settlements. Under these agreements, the Federal Reserve set up lines of credit for specified dollar amounts with each foreign bank, and in return received an equivalent line of credit in the foreign currency. At the end of August 1963 the total of these credit facilities outstanding was $1,550 million. Drawings under them had amounted to $978 million, repayments to $876 million. In addition, in 1962 the Treasury began to sell its own certificates and bonds, denominated in the respective *foreign* currency, to each of five European central banks and to the Swiss Confederation, receiving the foreign currency in exchange. The total of these issues outstanding in August 1963 was $705 million. Further, in July 1963 the Treasury arranged a stand-by credit of $500 million with the International Monetary Fund. The United States is also a party to the mutual assistance arrangements, totalling $6 billion and to be operated through the Fund, which were set up among ten leading countries at Vienna in 1961; and likewise to the so-called "gold pool" operated in London. The United States has in general been a leader in achieving the recent great increases in the exchange of information and in the degree of practical cooperation among the principal central banks—both admirable developments.

These financial devices were intended primarily, from the American side, to help combat any serious adverse speculation and possible panicky runs against the dollar. Their value was demonstrated spectacularly after the assassination of President Kennedy in November 1963. They were called into operation within a minute or two after the news broke, but the mere knowledge that such arrangements existed choked off most of the potentially enormous pressure against the dollar which might otherwise have developed, and made actual intervention on any substantial scale unnecessary.[9] But two things must be emphasized. First, the gains in international liquidity which these devices do or can provide are perfectly genuine, yet in a sense the gains are deceptive. Each actual increase in liquid assets which the United States

[9] See the remarks by President Alfred Hayes, of the Federal Reserve Bank of New York, before the meeting of the International Chamber of Commerce in New York on December 9, 1963 (mimeographed copies obtainable from the Federal Reserve Bank of New York).

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receives under them is necessarily accompanied by an equal increase in actual U.S. foreign liabilities—and except for the Treasury bonds, *short*-term liabilities—which must later be repaid.[10] Second, the establishment of these devices obviously does nothing whatsoever to solve the basic U.S. international-payments disequilibrium itself. From this latter point of view, they are and can be nothing more than a time-gaining buffer.

The actions and arrangements listed above constitute the principal measures which the U.S. authorities have thus far put into effect to combat our international-payments problems. None of them is very drastic. They point up sharply, however, the conflicts among its various economic and political objectives which the United States confronts.

A number of other measures have also been proposed by sources outside the administration, but most of them would likewise entail both conflicts of objectives and various undesirable ancillary effects. Thus in some American business circles, proposals for large increases in protective tariffs and in quota restrictions on trade have been viewed with favor. Even apart from the reversal of nearly all U.S. commercial policy declarations since World War II which such action would entail, however, and apart from its presumably adverse effects on the distribution of wealth and income, it would probably not accomplish the desired result. It would almost certainly provoke retaliation, and cutting the current supply of dollars to foreigners in this way would eventually affect U.S. exports unfavorably. The last two difficulties are also raised by the proposal to restrict foreign travel by Americans. Another suggestion has been to offer U.S. export firms extensive tax relief. But given the degree of the imperfections of competition in the United States, the adoption of this proposal might merely swell the profits of the firms affected, instead of producing cuts in export prices and (it would be hoped) a consequent expansion of exports them-

[10] It can hence also be argued that so far as used, the currency swaps and sales can have net deflationary effects. The receipt of foreign currencies under these arrangements will not produce internal expansion, since the currencies will be used to meet foreign demands for the conversion of dollars (into the demanders' or other foreign currencies, or into gold); but the eventual later repayment of the foreign currencies by the United States will work, other things equal, in the direction of domestic contraction.

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selves. Disadvantages also attach to a number of other smaller proposals, such as requiring that all government cargoes be shipped only in American-flag vessels.

III

More drastic than any of these are two proposals of a different sort, ones which the U.S. monetary authorities have thus far opposed flatly and vehemently, but which a good many academic economists have begun to favor: namely, to devalue the dollar sharply, and even to go over to a regime of more or less freely fluctuating exchange rates. These proposals require more extensive appraisal.[11]

At least on a first glance, both seem attractive. Given a persistent international-payments deficit, imposing the right amount of devaluation looks like a sure-fire device for abolishing the deficit and for preventing its recurrence. Indeed, changes in exchange rates are recognized in the very charter of the International Monetary Fund as being, under specified conditions, an entirely permissible way of dealing with a "fundamental disequilibrium" in a member country's balance of payments. A regime of freely fluctuating rates, on the other hand, if adopted by any substantial number of important countries, would presumably destroy both the Fund itself (by depriving it of any function) and most of the rest of the present international monetary structure. But it seems to offer a simple and automatic method for ensuring that the supply of and the demand for foreign exchange are always kept in adjustment through the mechanism of changes in the price of foreign exchange itself, and it solves the problem of international-payments deficits by abolishing it entirely. Indeed, it is doubtless the prospect of thus returning to the simplicity and purity of the classical universe, with its completely flexible prices, that has given this proposal, espe-

[11] The literature on the general theory of the foreign exchanges is very large. Among recent studies see the symposium on exchange-rate policies in *American Economic Review*, LIII (1963), 112–46; J. L. Stein, "The Optimum Foreign Exchange Market," *American Economic Review*, LIII (1963), 384–402; J. M. Fleming, "Domestic Financial Policies Under Fixed and Under Floating Exchange Rates," IMF *Staff Papers*, IX (1962), 369–80; E. Sohmen, *Flexible Exchange Rates* (Chicago, 1961); and E. A. Devletoglou, "Devaluation and the Stability of the Trade Balance" (unpublished Ph.D. dissertation, Columbia University, 1963).

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cially as formulated by the eloquent voice of Professor Friedman,[12] such a strong appeal to so many students.

Moreover, there is a good deal of historical evidence which seems to lend support to the general case for exchange-rate adjustment as a cure for a "fundamental" international-payments disequilibrium, and even a certain amount of evidence in favor of a regime in which exchange rates are left free to fluctuate (though presumably subject to control over abrupt short-run movements). But even the favorable evidence must be interpreted carefully, and there are numerous examples of failure. While it is obviously impossible to examine the whole historical record here, some summary comments may be offered on a few specific cases.

First, as far back as the end of the nineteenth century certain relatively underdeveloped countries, notably in Latin America, were at times able to use exchange depreciation as a method of meeting cyclical deficits in their international payments that were due to cyclical recessions in the more industrialized countries—the depreciations being followed by more or less equal appreciations later, when things had improved. But the problems of these countries were peculiar in several respects. Each of them typically exported only a few types of primary products to the industrial countries, while importing manufactured goods from the latter, and most of them also had to meet heavy fixed charges on previously incurred long-term debts abroad. When the industrial countries experienced domestic cyclical contractions the primary producers' exports to them fell sharply, and because the relevant demand schedules were both usually inelastic and had shifted far to the left, in the short run no export-price adjustment in terms of domestic currency could give much relief. Here exchange depreciation was perhaps the only practicable solution. Depreciation served, however, not to stimulate exports but rather to *reduce imports* heavily—thus helping to restore the countries' international-payments equilibrium. This experience is striking. Nevertheless, little of it seems applicable to the current problems of the United States.

Second, and on the other side of the discussion, after World War I many countries found themselves operating,

[12] See Milton Friedman, *Essays in Positive Economics* (Chicago, 1953).

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however unwillingly, under regimes of what were in fact quite freely fluctuating exchange rates. Few of these regimes were successful under any reasonable definition, and many of the currencies involved, including nearly all those of Central and Eastern Europe, soon depreciated to the point of worthlessness and disappeared. The chief explanation, of course, was the inability of the respective governments to control internal monetary and fiscal inflation; but in addition, after a time the fluctuating-rate regimes both encouraged and became peculiarly prey to adverse speculation. Third, the drastic British devaluation in 1931 was expected to produce an enormous improvement both in the balance of payments and in the severely depressed internal economy. But most of Britain's trading partners themselves devalued either at once or soon thereafter (the United States not until 1933), by roughly equivalent amounts, and the end result was to wipe out all or most of the British devaluation relative to their currencies. The swift return of prosperity within Britain after 1931 was due in some degree to the Ottawa tariffs of those years, but was due primarily to the relaxation of internal monetary restraints which the devaluation made possible and to other new domestic policies, not to the direct effects of the devaluation on British foreign trade. Fourth, only one of the several French devaluations between 1946 and 1958 was more than temporarily successful, again primarily because of the French failure to deal adequately with current internal inflationary pressures—in sharp contrast to the remarkable record of Italy after 1948.

Fifth, both the British devaluation of 1949 and the French devaluation of 1958 were successful, in the sense that the international-payments situation improved at once and that the new exchange rates were maintained. These were again cases in which internal price levels had previously got seriously out of line with the exchange rates, and in which—given the political impossibility of sufficient internal price deflation in both cases—devaluation was hence appropriate. But the devaluations *remained* effective because further internal price *in*flation was thereafter prevented or kept small, at least for several years. Sixth, the Canadian regime of (somewhat restrained) fluctuating rates in 1950–62 was successful in the sense that no wide swings in the rates and no

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severe balance of payments disequilibria developed.[13] But I think that the chief reasons for this success were again the maintenance of fairly effective monetary and fiscal discipline within Canada and—especially—the protracted and huge inflows of American capital, rather than the effects of any "automatic" adjustments in Canadian international price relations which could legitimately be attributed to exchange-rate changes. Finally, the small upward revaluation of its exchange rates by West Germany in 1961 succeeded in the sense that further expansion of Germany's international monetary reserves was checked. But the persistent German merchandise export surplus was hardly affected. Rather, the chief effect of the revaluation was to enable the German monetary authorities to let their interest rates fall, thus abating the excessive inflow of foreign funds.[14]

Drawing any generalizations from these and other historical cases is, of course, extremely risky, but two propositions may nevertheless be suggested for consideration. First, the key to the success or failure of deliberate devaluations in the more highly industrialized countries appears to have lain largely in the degree to which monetary and fiscal restraint was exercised subsequently. Where a "fundamental disequilibrium" had previously existed, devaluation may have been a necessary step for the restoration of equilibrium, but it was not a panacea (and this situation invites the reflection that had restraint been exercised sooner, devaluation itself might not have been required). Second, not only was this last proposition also true of regimes of freely fluctuating exchange rates but, in addition, in at least the Canadian case the success of the regime seems to have been due in considerable part to extraneous factors having little to do with the nature of the exchange-rate regime.

IV

Even if one regards the historical evidence as inconclusive, however, or as being merely not relevant to the present prob-

[13] The literature on the Canadian case has become substantial. See especially Paul Wonnacott, *The Canadian Dollar, 1948–1958* (Toronto, 1961).

[14] The simultaneous Netherlands revaluation likewise served chiefly to check further expansion of the Dutch international reserves. The merchandise trade balance, here always an *import* surplus, again showed little enduring change.

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lems of the United States, there are other grounds for being dubious about the effectiveness of devaluation as a solution for these particular problems. The various theoretical analyses developed in recent years to show the effects of devaluation on a country's balance of *payments* concentrate chiefly, and properly, on the balance of merchandise *trade* alone, and hence on the various relevant elasticities. Now it may be granted that in the case of the United States, the combined effect of these elasticities is likely to be favorable. In particular, price-elasticity "pessimism" is not warranted, unless in the very short run. But the arguments for U.S. devaluation at the present time which are or can be derived from these analyses also make certain other assumptions, whether explicitly or not, which are required if one is to conclude that the present U.S. international-payments disequilibrium can be greatly reduced or even eliminated by a sufficient degree of devaluation.

First is the necessary assumption that if the United States devalues, its principal trading and financial partners will not retaliate by devaluing more or less equally, thus nullifying the U.S. action. This much is obvious. But it is a precarious assumption to make with respect to the United States today. There is nothing to suggest that other countries, and least of all the Common Market group and the United Kingdom, would willingly accept the large increases in their imports from the United States, and the large decreases in their exports to the United States and to third countries, which would be precisely the main purpose of the U.S. devaluation. Nor would the countries which now hold commercial and "official-reserve" dollar balances and other dollar assets willingly accept any large decline in the value of these assets as measured in terms of their *own* currencies. Their most effective protection against such losses, obviously, would be to devalue themselves—a step which would also protect them against damage to their merchandise trade. Other countries might tolerate a small U.S. devaluation, if it were put in force after previous consultation and agreement, but not one of the size presumably required to meet the U.S. problem.[15]

[15] Also see Salant *et al.*, 254. Note that if all or most of the leading countries devalue by equal percentages, the inter-currency exchange rates will not be altered, but the national-currency prices of gold will rise proportionately. The resulting increases in the national-currency values of national gold re-

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Second, and even if there is no retaliation, the argument for U.S. devaluation must assume that despite the devaluation and the consequent increases in the dollar prices of U.S. *imports,* the dollar prices of U.S. *exports* and the general internal price level will not rise at all, or at least will not rise by nearly as much as the per cent of the devaluation. Otherwise the gains from the devaluation would be at best fleeting. This type of assumption is in general justified in the case of a country which has already had a serious internal inflation, whose prices are hence now "too high" relative to prices abroad, and which has therefore recently been undergoing adverse movements of its trade balance—all conditions which prevailed in the United Kingdom in 1949, for example, and in France in 1958. Here devaluation, if combined with adequate internal restraints, *can* correct the current balance of payments disequilibrium. But the United States is not now suffering from any of these conditions. There is no appreciable internal inflation or foreign-trade price distortion, and the trade balance has remained steadily favorable. In such a situation, a substantial devaluation can hardly fail to be followed by serious internal price increases —doubtless partly speculative, it is true—as in 1933–34. Any probable measures of monetary restraint would have a hard time trying to prevent, let alone reverse, such increases.[16]

Third, and even granting the two previous assumptions, there is another difficulty. The usual argument for devaluation looks to improvements in the balance of merchandise *trade* for the chief source of improvements in the total balance of *payments.* But the U.S. trade balance is already heavily favorable, as just noted, and has been so consistently on the average of recent years. The overall payments deficits have come primarily from other items, the bulk of which, as remarked earlier, are virtually immune to improvement through devaluation. To offset these deficitary items by making the trade balance still more favorable would require

serves, though in a sense illusory, may be thought to be "a good thing" for internal economic activity. The chief beneficiaries, however, will be the gold-mining countries—principally South Africa and the Soviet Union.

[16] Indeed, the end result might well be the necessity for the imposition of substantially more severe monetary restraint than had existed previously, and hence of more hindrances to internal economic growth, yet with little or no promise of any lasting reduction in the international deficit.

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the creation of a total excess of merchandise exports over imports which would have to amount to perhaps fifty per cent of the total of recent imports themselves. It seems clearly unlikely that so large a surplus could be achieved, or if achieved, that it could be maintained. Even if the trading partners of the United States did not retaliate with devaluations of their own, the internal pressures on them to raise tariffs and quotas against the United States would surely become formidable.

Fourth, on a different level of considerations, if it came to be generally felt that dollar devaluation was impending or was even being seriously considered, a panic flight of both foreign and U.S. funds out of the United States could easily start almost overnight, and if unchecked could sweep away all the current defenses of the dollar—including the relatively small excess of the total U.S. gold stock over present reserve requirements. Something like this came fairly close to happening in the winter of 1960–61. The only resort for the U.S. authorities would then be to impose exchange controls. Indeed, such a flight might develop even after an initial devaluation, from fear of still further devaluation later.

Finally, the U.S. Treasury, the Federal Reserve, and the late President Kennedy have all given unequivocal assurances that no devaluation will take place. Of course circumstances are alleged to alter cases, sovereign powers are sovereign, and even the most solemn apparently official commitments have sometimes been violated: consider the U.S. devaluation in 1933. But in the absence of some spectacularly large new adversity, it is hard to see how the U.S. administration can now change its position materially.[17]

These are the principal considerations which argue against the devaluation of the U.S. dollar today, in the special circumstances which the United States now confronts. I think that all or most of the same considerations likewise argue, *a fortiori,* against adoption by the United States of a regime

[17] In my view, the creation of exchange guarantees by the U.S. government would only make matters worse. The guarantees could not cover more than a part of the total of U.S. foreign short-term liabilities, let alone long-term foreign investments here; and their mere announcement would dramatize the crisis, and thus probably precipitate the very run on the dollar which they were supposed to prevent. For a more favorable view of guarantees, however, see John Williamson, "Liquidity and the Key Currency Proposal," *American Economic Review,* LIII (1963), 427–33.

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of freely fluctuating or even of merely "flexible" exchange rates, at least in these special circumstances.

There is also another argument against adopting the latter type of regime, as I see it: namely, that it would greatly increase uncertainty, and would hence reduce the time span over which private enterprise could rationally make international commitments for the future. Investment by foreigners in the United States would surely shrink greatly, as would foreign investment by Americans (the Canadian experience in the years after 1950 was surrounded by special circumstances, as remarked earlier, and I believe is not probative). Merchandise trade would be burdened by the greater riskiness to foreigners of credit extensions payable or receivable in dollars, and to Americans of extensions stated in foreign currencies, and by the consequently probable substantial increase in the average cost of forward cover; and the volume of trade itself would hence presumably shrink materially. Finally, the possibility of cumulatively destabilizing speculation like that which broke out against many currencies after World War I would always lurk around the corner.[18]

V

We have thus far reached conclusions which seem to be largely negative. It has been argued that in the special situation of the United States today no one of the three principal sets of remedial measures examined—the classical method of imposing severe monetary restraint, the other less drastic steps thus far taken by the U.S. administration, or currency devaluation—is likely of itself to cure the large and persistent international-payments deficits from which the United States has been suffering. Moreover, again in the special situation of the United States today, the first and the last of the three sets of measures could have seriously adverse side-consequences in other areas. Continued severe monetary and fiscal restraint would certainly hurt and perhaps halt economic growth and the reduction of unemploy-

[18] Salant *et al.*, 259, makes the interesting suggestion that exchange stability be maintained *within* a dollar-sterling bloc, and also *within* the European Economic Community bloc, but with exchange flexibility *between* the two blocs. But this arrangement is proposed only as a last resort, if all else fails.

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ment (and over time might even *increase* capital exports), while a substantial devaluation of the dollar against other leading countries, even if permitted by our trading partners, would probably induce price and wage inflation.

All this does not mean, however, that the case is hopeless and that nothing can be done; quite the contrary. The choice of steps to be taken naturally depends on the objectives which it is sought to achieve. Four such objectives seem to be of primary importance, and would presumably be accepted by the present U.S. administration as controlling: (1) the continued promotion of our internal economic growth and the continued reduction of unemployment; (2) maintenance both of the present defense posture and strength of the anti-communist nations, and of the flow of aid to the underdeveloped countries; (3) preservation both of the present value of the dollar in terms of gold and hence of the other gold-based currencies, and of the general character and framework of the present international monetary system, including continued use of the dollar as an important (but not necessarily the chief) international reserve currency; and, of course, (4) substantial elimination of the current huge international-payments deficits.

These objectives can be achieved simultaneously by the adoption of a set of five measures, only one of which requires the cooperation of other countries in joint action with the United States, and which in combination would substantially avoid the dilemmas and contradictions described in earlier pages. The considerations in favor of these measures seem self-evident, and need not be spelled out here. The measures are as follows: (1) adoption of the present tax-reduction proposals of the U.S. administration, and of its other major programs to stimulate internal growth and employment; (2) a return to monetary ease in the United States as rapidly as the balance of payments pressures make possible, to help toward the same goals; [19] (3) greatly increased partici-

[19] The issues involved here are complex. The recent rise in short-term interest rates, especially since mid-1962, has contributed substantially to reducing the outflow of short-term capital. But the continuance of monetary restraint, or even the mere absence of positive monetary ease, is hardly compatible with a continued stimulus to our rate of economic growth. This restraint should be reduced and terminated as rapidly as improvement in the balance of payments permits. What we face is hence in part a real conflict

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pation by the other "Western" industrial countries (including Japan) in the present mutual defense and economic aid programs—this step being recommended both in common equity, since all of the countries in question have a vital stake in the success of the programs themselves, and in order to help avoid the destructive effects on these countries which a collapse of the U.S. dollar would bring; [20] (4) if the U.S. deficit is not sharply reduced soon, the installation of machinery to appraise, and as far as necessary to restrict, *all* new private long-term capital exports by Americans, "direct" as well as portfolio; [21] and last of all, (5) if required in order to make control of capital exports effective, especially by preventing leaks through the short-term capital markets and through merchandise transactions, the installation of an appropriate form of exchange control. It does not now seem likely, however, that this last drastic step will be necessary.

Of these measures the third, already being attempted by the U.S. administration, has thus far not been viewed with enthusiasm by the relevant foreign governments, but its acceptance seems clearly to be in their own best long-run interests, economic as well as political. The fourth and fifth proposals, if put forward by responsible American authorities, will certainly evoke strong protests from parts of the American financial community. But the problems presented by the international-payments situation of the United States are grave, and are unlikely to be solved either by neglect or

of objectives, but in part (one may hope) only a problem of priorities of action in time.

Solution of the problem of unemployment is still more complex and difficult. Maintaining an enduringly higher level of general economic activity will help greatly, but is not the whole answer. Extensive programs for retraining workers displaced by technological progress, and a drastic improvement of the education offered children and teen-agers (who are the future working force), are clearly essential, but so is also—to bridge the gap until these measures can begin to bear fruit—the adoption of large new public-works programs, at both federal and local government levels.

20 Where relevant and possible, still greater efforts should also be made to increase procurement for the common defense and economic aid programs from United States sources, even when the funds involved are furnished by other countries. Some of the military procurement programs seem to be particularly suitable.

21 Such controls would be far preferable to the proposed interest equalization tax now before Congress and discussed earlier, because it would be both comprehensive and not irrationally discriminatory.

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by wishful thinking. Given the particular circumstances involved and given the unlikelihood of a fortuitous major change in the situation for the better, such as cannot now be foreseen or even hoped for, then the imposition of control over private long-term capital exports and if necessary of exchange control, when combined with the other measures proposed, are the steps which, if drastic action does become necessary, seem to offer both the best prospects for success in curing the deficit and the least likelihood of producing costly side effects. At least in these particular circumstances they seem far preferable to, for example, dollar devaluation.

VI

In conclusion, I should like to offer one further set of comments and suggestions. They relate primarily to the mode of operation of our "Western" international monetary system, though they also bear importantly on the problem of the U.S. payments deficits. To put matters in a nutshell, while I agree that the *general* character and framework of the present international monetary system should certainly be preserved, as proposed in the preceding section, I also think that the system as it now operates contains some fairly substantial defects and dangers, which should be dealt with promptly.

This last statement should not be taken to imply any disparagement of the new defenses for the U.S. dollar which have been built up recently, and which were discussed earlier. These defenses are at once powerful, flexible, and varied, and can continue to protect us for a considerable time against both speculative attacks on the dollar and the more immediate effects of possible future fears and panics. If our international-payments deficits continue on anything like the recent scale for any protracted period, however, it is obvious that these defenses can be gradually used up. Even today, under our existing laws and other arrangements the international liquid assets which are at present actually or potentially available to our monetary authorities amount to only some eighteen per cent of our total annual payments to foreigners—about 2.2 months' worth—and to less than one quarter of merely our total *short*-term liabilities abroad. This is only a rather limited protection.[22]

[22] The currency swap and other standby arrangements amount (October

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Moreover, what is in one sense even more important and as is familiar to all students, these latter demand and other short-term liabilities to foreigners also provide a substantial though varying part of both the international and the domestic central monetary reserves of most of the other countries in the "Western" international monetary system. From the point of view of these latter countries, our short-term *liabilities* abroad are their liquid dollar *assets*. This is where the trouble comes in, for two results then follow.

First, as our international-payments deficits and hence our short-term liabilities abroad grow, so do (in general) foreign monetary reserves. But if we get into real balance of payments difficulties, so that our ability to repay (convert) these liabilities into gold or foreign exchange seems to be threatened, then the monetary stability and even the solvency of the foreign countries that hold these liabilities can also be imperilled. Conversely, if in the future we manage to reverse our course and turn our present international deficits into surpluses, our short-term liabilities abroad will shrink. But so will foreign monetary reserves, thus conceivably threatening the international liquidity of other countries and even forcing either severe internal contraction or exchange controls on them (as happened after 1945). Second, *even if our balance of international payments itself were previously in equilibrium,* a sudden decision by foreign monetary authorities to withdraw their short-term dollars on a large scale by converting them into gold or into other currencies could at any moment seriously threaten the stability of the U.S. dollar itself. And all of these same things are also true, though to a much more limited degree outside the sterling area, with respect to the other principal reserve currency, the British pound.

1963) to over $2 billion; U.S. holdings of foreign currencies to $270 million; and our gold stock in excess of reserve requirements to roughly $3.6 billion. The total is roughly $6 billion, a sum which would cover our average recent international-payments deficits for less than two years. Also, as noted earlier, our short-term liabilities to foreigners are now some seventy per cent larger than our *total* monetary gold stock. On the other hand, however, the suspension or abolition of our present laws on minimum gold reserves could make the whole of this gold stock available for use in making payments abroad, not merely $3.6 billion of it; and the additional resources which we could borrow from the International Monetary Fund and from other nations are presumably still pretty large. Our situation must hence be taken seriously, but to date is hardly critical.

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This is *prima facie* an undesirable situation. The internal monetary stability and the international liquidity of other countries ought not to be significantly dependent on the accident of the current international-payments deficits or surpluses of any one other country, not even of the United States; and the international stability of the U.S. dollar ought to be dependent chiefly on what we ourselves do currently about our own international payments and receipts, and not in any important degree on the possibly adverse decisions and actions of foreign monetary authorities—who must after all be guided primarily by their estimates of their own nations' best interests. Indeed, it can be argued with some plausibility that in recent years the "Western" international monetary system has really been kept free from major disturbances by a combination of emergency measures, perhaps some good luck, and the general reluctance of other countries to risk disaster to themselves by putting too much pressure on the U.S. dollar and the British pound. But this combination is surely not good enough to serve as the principal basis for an enduring international system. What is at issue here is not simply a matter of sufficient or insufficient international "liquidity," but clearly something more fundamental.

There are probably a number of different ways of improving the operation of the present international monetary system. The solution that seems to me most likely to achieve the desired goals, however, is to convert the International Monetary Fund into an international central bank and to use the deposit liabilities of the new bank, to the extent desired or necessary, as the medium for making net settlements of international payments-balances.[23] The adoption of this type of arrangement would at once free most foreign monetary systems from their present degree of dependence, which is sometimes dangerously direct, on general confidence in the U.S. dollar and its stability; and would also free them from

[23] The initial proposal in this direction was of course made by Professor Triffin. See especially his *Gold and the Dollar Crisis* (New Haven, Conn., 1960). Among other recent studies and comments, see my article, "The Reorganization of the International Monetary System," in the *Economic Journal*, LXXI (1961), 691–708, and Salant *et al.* It would probably be convenient, though not essential, to denominate the new deposits in terms of some new currency unit.

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their potentially excessive sensitivity to fluctuations in the U.S. balance of international payments. The foreign countries involved would or could hold the new IMF deposit balances, where they now hold dollars; U.S. international-payments deficits would, to the extent desired, increase the holdings by these countries of IMF balances, rather than of dollars alone; and the contractive effect on them of possible future U.S. payments surpluses could be easily offset, under appropriate safeguards, by extensions of IMF credits beyond the limits now possible. The United States would also be freed from the dangers, now potentially great, of sudden international runs against the dollar initiated by other nations.

It should be emphasized that the reciprocal currency swaps and standbys which have been set up in the last two years by the U.S. administration and the more restricted "Vienna" agreements for mutual financial assistance through the present IMF which were established slightly earlier, when viewed as a combined total of international arrangements, bear a much closer resemblance to the proposed new international central bank than is generally recognized, especially by the opponents of even the mere idea of such a bank. Indeed, these arrangements are really a sort of half-way house to the new bank. But they are made up of a series of what are essentially emergency rather than permanent measures, and many of them could be abruptly terminated on short notice; the total volume of resources which they involve—their total financial power—is substantial, but still relatively limited; and they are all set up in terms of specified national currencies. The proposed new bank, on the contrary, would be a permanent institution, not a temporary and quickly revocable emergency device; it could have very large resources; [24] it would operate on the basis of a true pool of all its members' currencies, not as a set of largely bilateral national currency agreements (which is what much of the present systems of supplementary currency defenses really comes to); and a country which had received title to the new IMF deposits in payment for another country's debt would no longer have to worry about the liquidity—let alone the solvency—of the

[24] At the minimum, the new bank's resources would consist of the present assets of the IMF plus a presumably substantial part of the present foreign-exchange reserves of its members.

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debtor's currency. To the extent desired or necessary, and subject to specified safeguards, the new bank would in effect be interposed as a sort of buffer between debtor and creditor countries on their current international-payments accounts. The creditor country could receive IMF deposits in payment of the short-term debts owed to it, if it wished, instead of receiving the perhaps increasingly suspect debtor-country currency; and the debtor country would then owe much or all of the critical part of its foreign short-term liabilities to the IMF, rather than to a creditor country that at any moment might suddenly become apprehensive or even panicky about the debtor's own currency. In such a situation, the IMF might insist that the debtor take drastic measures of reform, but it would not initiate a run against the debtor's currency. Finally, when the "supply" of international liquidity again becomes a serious problem, as it almost certainly will, the new bank can provide an adequate solution, yet with satisfactory and acceptable safeguards against abuse.

Other reasonable and workable methods for improving the operation of our present international monetary system have also been proposed, but they will not be explored here.[25] The central point, as I see it, is that *some* pretty substantial improvement must be made fairly quickly if the system itself is to have a reasonably good chance of continuing to avoid such near-disasters as those which threatened in 1960–61 (and which were forestalled only by drastic emergency improvisations), and of contributing to rather than imperiling the long-run internal prosperity and the monetary stability of its members. We should be delighted by the spectacular initial successes of the recent measures to defend the dollar, and by the recent encouraging surge in American internal economic activity, but we should not be lulled by them into indifference and inaction. The basic problems are still there, and are still dangerous.

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25 Also see F. Machlup, *Plans for Reform of the International Monetary System* (Princeton, 1962); R. V. Roosa, "Reforming the International Monetary System," *Foreign Affairs*, XLI (1963), 107–22; and H. G. Grubel (ed.), *World Monetary Reform* (Stanford, Calif., 1963).

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NATIONAL POLITICAL ALIGNMENTS AND THE POLITICS OF LARGE CITIES

BIG city politics and presidential politics are commonly thought to be closely related, but the details of the relationship are obscure. Here an attempt is made to illuminate them by historical and comparative analysis of elections in large, northern partisan cities.[1] Many of our larger cities, mostly west of the Mississippi, hold nonpartisan local elections; and they figure in the discussion only occasionally and for comparative purposes.[2]

There are several common propositions about the interrelation of national and big city politics. One is that our national politics was progressively "urbanized" in the nineteen-twenties and thirties; that issues and programs of interest to urban "minorities" came to the fore in this period and significantly altered both the subject matter and structure of national politics. An allied proposition is that Democratic strength in presidential elections came critically to be based on the large cities. A more specific form of this thesis asserts that Democratic presidential victories have depended upon the efforts of party "machines" or "organizations" in the large cities; that is, that more than the personal party preferences of city dwellers is involved. A final proposition amounts to the converse of the foregoing: that local Democratic electoral dominance and/or organization control in our large cities is largely a function of party preferences based on *national* politics.

These propositions may all be true; they are not neces-

[1] The principal cities in the analysis are listed in Table 1 below. This discussion is drawn from a broader study of twenty-four of our largest cities based primarily upon election returns and newspaper reports of campaigns and elections, supplemented by the available periodical and monographic literature. All cities of 500,000 or more by the 1950 Census (save Washington) were included, plus seven cities of 300,000–500,000 selected on regional considerations and for availability of data.

[2] Actually, four large cities that are formally nonpartisan in some or all local elections, but which are *de facto* more-or-less partisan, are included in this discussion: Chicago, Cincinnati, Cleveland, and Kansas City.

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sarily inconsistent, especially if long-run and short-run effects are distinguished. In this article the relations between presidential and municipal elections are examined first in the "long run," which is taken as the period since the advent of the New Deal; and the level of Democratic dominance in most of our large northern cities is examined historically to try to identify the major influences at work. Within this span the years before and after 1944 (when Democratic presidential margins began seriously to decline) are taken as "short-run" periods. It cannot be claimed that these "long-run" and "short-run" periods are typical of others, since they were probably unique. The dating of the periods, or of the long run and short run, must be rather arbitrary; but it is hoped that the periods are appropriate for the purposes of this discussion. The main purpose is examination of the propositions set out above. The conclusions do not overturn what seem to be the accepted views; but they do suggest some qualifications and distinctions—they force one to refine the propositions. Finally, it is hoped that the historical and comparative discussion below will be worthwhile for its own sake, since the electoral behavior of our large cities has not received much of this kind of treatment and our published knowledge of it fragmentary.[3]

I

This section contains a brief review of political changes in the large partisan cities since the nineteen-twenties. City election returns in presidential and local elections are set out in Table 1 for reference during the discussion.

The growth of urban Democratic alignments in national politics in the twenties and thirties is well known,[4] as is the increasing importance of Democratic city pluralities in presidential elections.[5] More partisan cities could be described

[3] Newspaper and periodical reports of campaigns and elections have been read from 1920, where they are available. Neither "hard" data nor election chronicles are as available for the prewar as for the postwar period; and there is less material for some cities than for others. Every effort has been made to collect data and accounts relevant to *crucial* elections or episodes.

[4] See Samuel Lubell, *The Future of American Politics* (New York, 1952), especially Chaps. 1–3.

[5] See Samuel Eldersveld, "The Influence of Metropolitan Party Pluralities in Presidential Elections since 1920," *American Political Science Review*, XLIII (1949), 1189.

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TABLE 1

DEMOCRATIC PERCENTAGES IN PRESIDENTIAL AND LOCAL ELECTIONS [a]

Cities	1928		1932		1936		1940		1944		1948		1952		1956		1960	
	P	L	P	L	P	L	P	L	P	L	P	L	P	L	P	L	P	L
Baltimore	48.3		67.0		68.3		64.0		59.2		53.3		51.7		44.1		63.9	
		69.0		64.0		56.2		42.5		57.2		64.6		55.7		67.9		49.0
Buffalo [b]	46.6		48.2		58.3		54.5		55.6		56.8		49.6		42.3		64.9	
		47.5		54.6		50.3		50.2		42.9		44.9		50.8		50.0		
Chicago	46.9		57.1		66.9		56.5		61.4		58.6		54.4		48.7		63.6	
		58.3		70.8		56.2		54.7		58.7		56.1		54.9		71.4		55.6
Cincinnati [c]	42.7		50.9		59.2		49.9		49.7		48.3		43.2		37.5		50.4	
	63.7	60.0	54.3	58.8	56.0	51.0	40.0	46.0	45.0	52.1	52.8	47.6	51.4	48.6	49.7	60.0		41.5
Cleveland	46.1		52.8		76.5		69.9		67.9		61.8		59.9		54.6		70.9	
		53.3		45.0		43.2		57.6		67.0		61.5		59.9		69.5		73.8
Indianapolis	40.1		52.1		59.0		49.6		48.6		48.3		41.2		40.8		47.0	
		62.5		54.7		51.8		47.5		53.2		44.4		56.1		57.4		
Kansas City	43.3		67.5		74.3		57.6		55.0		61.2		51.9		53.1		55.7	
		58.2		63.4		61.2	44.0	38.9	33.9	44.9	43.5	44.9		33.3		57.1		49.0
New York [d]	62.1		71.4		75.4		61.2		58.9		51.1		54.4		50.9		62.8	
		61.0	52.0	27.0		40.0		46.0		57.0	49.0	36.0		46.0		68.0		51.1
Philadelphia	39.7		44.0		62.1		60.0		58.9		49.6		58.4		57.0		68.1	
		7.8		46.5		48.0		44.9		43.8		58.1		59.3		65.6		
Pittsburgh	42.7		55.5		70.7		61.6		60.8		60.1		56.1		52.3		67.0	
				59.2		57.0		50.8		53.1		61.4		62.6		64.3		66.6
St. Louis	52.2		64.7		67.0		58.1		60.4		64.3		62.0		60.9		66.6	
		48.3		52.6		58.8		44.6		33.3		54.4		63.7		76.8		62.9

[a] Local elections are city-wide elections, for mayor in most cases except in Cincinnati.

[b] Buffalo Democratic percentage is of two-party vote; minor party votes have sometimes resulted in winners without absolute majorities.

[c] In Cincinnati, percentage is non-Republican vote—combination of Charter party, independents, and Democratic since 1957.

[d] New York City percentage is for Democratic candidate against all others.

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as *locally* competitive in the twenties and thirties than can be so described today; and no partisan cities have become *more* closely competitive in general elections despite the oft-remarked erosion of the bases of "machine" politics since that time. All of the cities that have become less competitive since the nineteen-thirties have tended in a *Democratic* direction; and, in contrast to the period of the nineteen-twenties, none of the partisan large cities today is normally Republican, and only one or two are closely contested with any frequency.[6]

It thus appears that, in the long run, local electoral behavior in the partisan cities has been heavily influenced by national electoral behavior and attitudes—that "party identifications" primarily rooted in national politics have resulted in Democratic dominance locally and have reduced the incidence of local party competition.

While the foregoing statement of the basic national-local relationship is probably correct, the review that follows reveals some qualifications and complications of it. A summary statement here will add point and clarity to the narrative:

(1) Despite the trend in presidential voting, *local* Democratic electoral dominance was long delayed in some large cities, indicating the independent role of local conditions.

(2) There were significant differences in the strength and effectiveness of political organization from one city to another which affected the timing of party turnover.

(3) Another important local condition was municipal corruption and its exposure; party turnover was often the immediate result of public scandal.

(4) The Depression (entirely apart from the New Deal) had a serious effect on city political organizations, *both* Republican and Democratic; dominant Democratic organizations in such cities as Baltimore and New York were shaken severely and never fully recovered. In such cities, as one would expect, competition was most effective and organization control least effective in city-wide elections, where city councils were elected by districts.

[6] Cincinnati today is Republican both nationally and locally; but it is a formally (if not effectively) nonpartisan city, and prior to the demise of Proportional Representation in 1957 was very closely contested. The local Charter party often held majorities in Cincinnati's city council.

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(5) There was a sharp recession in local Democratic strength in some large cities in the late thirties and early forties coincident with a decline in Democratic margins in national elections.

(6) In the period since 1944, Democratic local electoral strength has been consolidated in most large cities and has become quite impervious to short-run trends in presidential elections. Local politics in large partisan cities are, generally speaking, less competitive than they were in the prewar period.

(7) The history under examination here can therefore be divided into two periods, roughly prewar and postwar, with the years of World War II a very broad dividing line. The differences between these periods are discussed in Section II; and the party organizational differences indicated above are examined in Section III.

That the long-run relation between presidential and city politics has been dominated by the presidential—that is, by national party identifications—is indicated by a brief review of long-run local change in the large partisan cities since the nineteen-twenties. Only two northern partisan cities in Table 1—Baltimore and New York—may be said to have been dominantly Democratic in the nineteen-thirties.[7] After a local Republican threat in the late twenties and Hoover's capture of the city by a bare 10,000 votes in 1928, Baltimore went more solidly Democratic than ever in the thirties, though the Democratic party was subject to more serious and persistent factionalism than it was before the Depression and New Deal.[8] Although the Republicans elected a mayor in 1943 against a four-term incumbent, they carried but one councilman after 1931, and none at all after 1939. When, in

[7] Baltimore elected a Republican mayor twice between World War I and the Depression, and from 1927–31 had a Republican mayor and a council evenly divided between the two parties. The council was normally heavily Democratic, and the two Republican mayoral victories were at least partly creditable to Democratic organizational sabotage of a non-"organization" candidate who won the primary (1919) and to reaction against a Catholic Democratic candidate (1927). Since 1926 more than sixty per cent of Baltimore's voters have regularly been registered as Democrats.

[8] The reasons for this are discussed in Edwin Rothman, "Factional Machine Politics: William Curran and the Baltimore City Democratic Organization, 1929–1946" (unpublished Ph.D. dissertation, The Johns Hopkins University, 1946).

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1963, the same Republican, McKeldin, again won the mayoralty with heavy newspaper support and factional Democratic defections from a much criticized nominee, the Council, elected by districts, remained wholly Democratic.

In New York, Mayor LaGuardia's success in city-wide elections cannot be credited to conventional Republicanism: the Depression, the Seabury investigations, LaGuardia's social-economic liberalism and support of President Roosevelt, Roosevelt's support of LaGuardia, LaGuardia's successful promotion of "Fusion" and later reliance on the American Labor party, his personal qualities and calculated appeal to minority groups were all elements of electoral success. Throughout his administrations the City Council remained Democratic, though the Fusion forces controlled the Board of Estimate after 1937. In the postwar period New York again went solidly Democratic, though not always "organization" Democratic at the city-wide level; and, in fact, organization "control" appeared to be weakening despite the continued strength of Democratic voting.[9]

Two other partisan cities in Table 1—Chicago and Pittsburgh—were relatively competitive and regularly Republican, respectively, in the nineteen-twenties. Hoover narrowly carried both in 1928. Chicago, after a close and factionalized party balance in the twenties, went Democratic in 1931 in response to the Depression, and the uninterrupted Democratic dominance of the city since 1931 is well known.[10] Pittsburgh, after a lengthy period of Republican hegemony, elected a Democratic mayor and council majority in 1933.[11]

9 Compare W. Sayre and H. Kaufman, *Governing New York City* (New York, 1960), 180–202, for a detailed discussion of the strengths and weaknesses of New York's Democratic organization in city elections; and F. Shaw, *History of the New York City Legislature* (New York, 1954) on the City Council in the LaGuardia period.

10 Chicago's city council was Democratic throughout the nineteen-twenties save for an evenly divided council, 1929–31, indicating the strength of the Democratic organization in the wards. Both Republicans and Democrats elected mayors in the nineteen-twenties. For a discussion relating the 1931 election to the Depression, see H. Gosnell, *Machine Politics* (Chicago, 1937), Chap. 1.

11 Republican control of Pittsburgh in the nineteen-twenties was tight and effectively unchallenged, as it had been for years. In 1933, however, indictment of the Republican mayor for malfeasance and the developing disaffection of an independent Republican group (which beat the organization in the 1931 primary) probably contributed to the Republican defeat.

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Democratic dominance rapidly increased; by 1936 the Democrats held a registration majority for the first time since the Civil War, and since 1939 no Republican has held elective city office in Pittsburgh (despite close elections in the late thirties and early forties).

Four cities in Table 1 seemed safely Republican in the twenties, consistently returned Democratic presidential majorities from 1928 or 1932 (1936 in the case of Philadelphia) until at least 1948, but, while they became more closely competitive or even leaned Democratic in local elections in the thirties, Democratic dominance was delayed until the postwar period. Then it came by gradual drift (in Buffalo and Cleveland) or by dramatic reversals (in Philadelphia and St. Louis). The apparent reasons for this behavior will be discussed shortly.

Finally, there are two of our partisan large cities which are not dominantly Democratic today. Cincinnati (which is formally but not effectively nonpartisan) was closely contested by Republicans and the local Charterites until the end of Proportional Representation in 1957; it is now at least leaning Republican locally and might better be described as Republican-prone. It went Democratic presidentially in 1932 and 1936 but not thereafter until it barely did so in 1960.[12] True, Cincinnati is formally a nonpartisan city, and the Democrats merged in the mid-twenties with the local Charter party; but the long-run maintenance of this merger (up to 1957) at least suggests that the ephemeral trend to Democratic presidential voting in Cincinnati did not involve changing personal party identifications or enable the local Democrats, on their own, to challenge effectively the strong Republican organization and tradition.[13] Indianapolis has

[12] Like most other large cities, Cincinnati was subject to the falling-off of urban Republican electoral strength in 1928 (though not throughout the twenties) described in Eldersveld; but Cincinnati's Republican presidential margins in the early twenties were far more lopsided than those of most large cities.

[13] On Cincinnati politics, see R. Straetz, *PR Politics in Cincinnati* (New York, 1958). I have been unable to ascertain whether the Cincinnati Democrats seriously considered striking out on their own locally in the early thirties. Kansas City is similar to Cincinnati in that formally nonpartisan elections have made possible the creation of a local party to compete with the established political organization; but in this case the Democrats were in power in the nineteen-twenties and thirties down to 1939, when the Reform party ousted the organization (or congeries of organizations) and remained in

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had more-or-less competitive local politics since 1929. Safely Republican in the twenties, it was governed by Democrats in the thirties; but election margins were not lopsided throughout the thirties, and there has been rather regular party alternation since 1943 although the city might be said to be leaning Democratic today.[14] Like Cincinnati, Indianapolis went Republican for president throughout the twenties and narrowly Democratic in 1932 and 1936, but it has since been consistently Republican in presidential elections.

Thus, to sum up, the two cities not now dominantly Democratic in local elections are the only two not carried by the Democrats in presidential elections after 1936; that is, they are the two with the strongest tradition of Republican presidential voting. Both cities became *competitive* both presidentially and locally (though local competition in Cincinnati probably depended upon formal nonpartisanship, PR, and the local party device). Two cities (Baltimore and New York) were already normally Democratic both presidentially and locally in the twenties. Despite the LaGuardia phenomenon, it may be said that both remained dominantly Democratic locally after 1932, and that Baltimore became more so after a Republican threat at the end of the twenties. Two cities (Pittsburgh and Chicago) were Republican or roughly competitive in the twenties, and both quickly became dominantly Democratic locally. One—Chicago—was already competitive with strong (often warring) Democratic organizations in the wards, but the Republicans were gaining in the late twenties. In Pittsburgh the rapid shift to Democratic

power to 1959. A number of Democrats participated in the Reform party just as reforming Republicans worked in Cincinnati's Charter party. The Kansas City history may seem an exception to the argument being developed here; but the effect of nonpartisan elections, the factionalism of the local Democratic party together with its excesses, and its temporary return to power in 1959 appear to disqualify it as a serious exception. See A. T. Brown, *The Politics of Reform: Kansas City's Municipal Government, 1925–1950* (Kansas City, Community Studies, Inc., 1958); M. Mulligan, *Missouri Waltz* (New York, 1948); and W. Reddig, *Tom's Town* (New York, 1947).

[14] The politics of this city and of the state of Indiana have historically been competitive. Indianapolis seemed safely Republican in the nineteen-twenties, although the role of the Ku Klux Klan in the Republican party may, in the end, have alienated more voters than it attracted. The election of a Democratic city government in 1929 followed the conviction of the Republican mayor and state governor of bribery and political corruption and the interim appointment of a Democratic mayor by the Republican city council in 1927.

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dominance seems to have been aided by a rift in the Republican organization, its more-or-less discrediting through the public discovery of corruption in 1933, and a local tradition of (often factional) machine politics.[15] Organized labor also played an active, auxiliary role in Pittsburgh.

Finally, four cities—formerly Republican nationally and locally—began to go Democratic for president in 1928, 1932, or 1936, and became, on the whole, competitive and *more* Democratic locally but not *dominantly* Democratic. In each city certain local factors conditioned and delayed the impact of presidential politics upon city politics. Buffalo and Cleveland have in common the lack of a modern tradition of cohesive, city-wide party organization. The formal nonpartisanship of Cleveland city elections favors insurgency and independence in primary elections; and the vigorous nationality politics of each city makes organization discipline difficult and occasions frequent crossing of party lines.[16] In Cleveland, the nonpartisan primary, the personality and reputation of Harold Burton, continued Republican control of the Negro vote in the "machine wards," and serious Democratic divisions in which the majority faction lacked New Deal and labor support all helped elect Republican mayors and council majorities through the nineteen-thirties.[17] The gradual growth of Democratic electoral dominance began in

[15] In its heyday the Pittsburgh Republican party was recurrently threatened with factionalism until, in 1925, Secretary of the Treasury Mellon dictated a "harmony ticket." The minority Democrats were similarly factionalized, and remained so until the postwar period. Many Pittsburgh professional politicians are reported to have changed parties in the thirties.

[16] On the volatile, anti-organization tradition in Cleveland politics, see R. L. Maher, "Cleveland: A Study in Political Paradoxes," in R. S. Allen (ed.), *Our Fair City* (New York, 1947). Party lines and affiliations are patent in Cleveland elections, and the city cannot be called effectively nonpartisan. Buffalo held nonpartisan elections during most of the nineteen-twenties; its nonpartisanship was no more effective than Cleveland's, though it seems similarly to have favored insurgency.

[17] Democratic factionalism (partially based on the "organization's" failure to support Roosevelt at the 1932 convention) led to defeat of the incumbent Democratic mayor in 1933. In 1935 Harold Burton eliminated the Republican organization incumbent in the primary and won the general election in an "anti-machine" campaign in which he also promised to deal more firmly with organized labor and city finances. By the late thirties Democratic organization and opposition were negligible; and Burton's general election opponent in 1939 was another Republican, supported by organized labor and the vestigial Democratic "machine."

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1941 after Burton's withdrawal and the advent of Frank Lausche, an independent Democrat with strong ethnic appeal; but city-wide organization control has never been achieved.[18]

In Buffalo the Democrats narrowly won control of the Council in 1931 and the mayoralty in 1933 and retained them in close elections through the thirties, but the Republicans regained control in the early forties. In the nineteen-fifties demographic movements and council redistricting resulted in Democratic city control despite Republican presidential majorities, but city-wide electoral margins have remained close.[19] Nationality has perversely and pervasively affected Buffalo's politics: the Republican recovery of the forties was partly based on their turning to the Polish bloc for candidates and on Italian foreign policy resentments; and the large Polish vote went Republican for president in the fifties on foreign policy issues. But Buffalo's history of persistent insurgency and disregard of primary election outcomes dates back at least to World War I; it appears that nationality identifications have reinforced an antecedent condition of party infirmity which has also been favored by a one-term limit for mayors and at-large councilmen.[20]

Philadelphia entered the Depression with a Republican organization at least as dominant as that of Pittsburgh. This organization survived the New Deal in the wards and divisions; but the city leadership was shaken, and control of the party changed hands in 1934 and again in the early forties. The party was unable to deliver presidential majorities after 1932; mayoral elections in 1935 and 1939 were hard fought and narrowly won, and the Democrats prevailed in the off-

[18] The recent Mayor Celebrezze, capitalizing on his own national origin and his appeal to the "cosmopolitan" vote (the Cleveland *Plain Dealer's* term), eliminated the Democratic organization's well-qualified candidate in the 1953 primary and went on to win.

[19] In 1961 a Republican won the mayoralty when the larger Democratic vote was divided between two candidates of different nationality backgrounds. The winner, Koval, had long held city office and had closely contested the mayoralty in the past.

[20] The importance of minor parties and occasional insurgent factions in Buffalo elections probably reflects and reinforces this tradition, though it is also conditioned by the New York election law. In the early years of this century Buffalo city elections were formally, if rather ineffectively, nonpartisan; this, too, may have fostered a tradition of insurgency.

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year elections of 1933 and 1937; but the Republicans recovered in the early forties.[21] Late in the forties, as the Democratic organization continued to build, a series of city scandals broke which, reminiscent of Pittsburgh, brought about a party reversal in 1951. The quick trend to local Democratic dominance seems to have resulted from the tradition of Democratic presidential voting since 1936 and the strength of the minority Democratic organization which this made possible, as well as from prolonged malversation and peculation in city government.[22]

In St. Louis, too, corruption and scandal played a role. Republican in the twenties (though in closely contested elections), the city went Democratic for mayor and council in 1933 and lopsidedly so in 1937, and it seemed on its way to Democratic dominance under the influence of heavy presidential majorities. But the Missouri "governorship steal" of 1940–41, together with the local execesses of the Democratic organization, gave the Republicans an opportunity; they won the mayoralty and council control in 1941 and again (by landslides) in 1944 and 1945.[23] Negro defection over insufficient "recognition" contributed to the Democratic decline. The postwar Democratic recovery began in 1949, and seems traceable in part to Republican factionalism and another Negro realignment. Democratic electoral domi-

21 The 1934 change should probably be attributed to the decline and demise of William Vare as well as the ascendancy of Roosevelt, though there had always been factionalism in the local Republican organization. See J. Salter, "The End of Vare," *Political Science Quarterly*, L (1935), 215; and J. Reichley, *The Art of Government* (New York, 1959), Chap. 1. The 1935 primary was an "open" primary, won by a long-time opponent of the organization (who had won the controllership in 1933 as a *Democrat*). As Mayor, Wilson again turned Democratic and persistently feuded with the Republican city council up to his death. All this the Republican organization survived, together with the virtual bankruptcy of the city and its major services by 1940 and a general reputation for thoroughgoing corruption.

22 The Roosevelt victories (and temporary Democratic control of the state) stimulated Democratic organization under new leadership. John P. Kelly, the principal organizer and mayoral candidate in 1935, had been a Republican, as had several other leaders. In 1935 the Democratic organization was described as modeled after the Vare machine and just as strongly controlled. *The New York Times*, September 15, 1935, Sect. 2, 1. After 1951 many professionals switched sides. The transition to Democratic control is described in Reichley.

23 On the "governorship steal," see C. F. Hurd, "St. Louis: Boundary-Bound," in Allen, 237 and *passim*.

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nance of St. Louis rapidly increased after 1949, although the Democratic organization suffered from factionalism and some hard-fought primaries, and a majority of the city's wards were weakly organized.[24]

This brief and selective history seems to support the general proposition that party identifications mainly oriented to presidential elections have led the way to *local* Democratic dominance, and that in this sense national politics have come to dominate the politics of the large partisan cities in the long run. Two further aspects of the post-World War I history support the proposition in this qualified, long-run form. One is the fact that several of our large partisan cities were somewhat more competitive in the twenties than they are today, despite the then Republican dominance in national elections. In Buffalo, Indianapolis, and St. Louis city-wide elections were occasionally close and Republican control was not secure; and the same comments apply to Democratic control in Baltimore and Chicago. One reason for this seems to have been the rise of disturbing and distinctively "urban" issues in city politics in the twenties—chiefly ethnic and religious competition and conflict, together with the prohibition issue in some cities. It has been argued that the "nationalization" of these issues resulted in the big city trend into the Democratic column in national elections that had set in by 1928.[25] Thus, we might expect to find a significant association between the proportion of ethnic and religious minorities in the urban population and the Democratic percentage of the presidential vote, though there are many other influences to be allowed for.[26] Table 2 shows, for twenty large northern cities, the Census figures for percentage of foreign-born white population in the Census years 1900–30; the average for those four years; the percentage

[24] On the factional and fragmentary character of political organization in St. Louis in the nineteen-fifties, see R. H. Salisbury, "St. Louis Politics: Relationships Among Interests, Parties, and Governmental Structure," *Western Political Quarterly*, XIII (1960), 498.

[25] See Lubell, Chap. 3.

[26] The degree and directness of association between the proportion of urban minorities and percentage of Democratic presidential voting would presumably be affected by such things as local and state political traditions and political organization, and by differences and rivalries among the minorities themselves, *inter alia*.

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TABLE 2

CENSUS DATA FOR SELECTED CITIES

City	I Percentage Foreign-Born White				II	III	IV	V	VI
	1900	1910	1920	1930	Average Foreign-Born White	Percentage Negro 1930	Mean Democratic Per Cent for President 1928–1940	Mean Democratic Per Cent for President 1956–1960	Per Cent "Minority" [a] by 1960 Census
New York	36.6	40.4	35.4	33.1	36.4	4.7	67.5	56.9	63.3
Boston	35.1	35.8	31.9	29.4	34.1	2.6	67.4	64.1	55.3
Chicago	34.4	35.7	29.8	24.9	31.2	6.2	57.4	56.2	59.5
Cleveland	32.5	34.9	30.1	25.5	30.8	8.0	61.3	62.8	59.8
Detroit	33.7	33.6	29.1	25.5	30.5	7.7	57.1	66.4	61.4
San Francisco	30.4	31.3	27.1	24.2	28.3	1.3	63.2	53.1	61.9
Milwaukee	31.1	29.8	24.1	18.9	26.0	1.3	69.8	54.6	38.9
Buffalo	29.5	27.9	24.0	20.6	25.5	2.4	51.9	53.6	48.9
Minneapolis	30.0	31.3	27.1	24.2	24.8	1.0	54.2	50.5	34.9
Seattle	23.1	25.6	23.4	20.2	23.0	0.6	55.2	46.5	17.8
Pittsburgh	26.3	26.3	20.4	16.3	22.3	8.2	57.6	59.7	47.1
Philadelphia	22.6	24.6	21.8	18.9	22.0	11.3	51.5	62.6	55.8
Portland	19.6	21.1	18.2	16.0	18.7	0.5	57.0	48.6	31.1
Los Angeles	17.4	18.9	15.9	14.7	17.5	3.1	55.3	52.6	49.4
Denver	18.9	18.2	14.1	10.9	15.5	2.5	52.5	51.1	25.6
St. Louis	19.2	18.2	13.3	9.8	15.1	11.4	60.5	63.8	42.9
Cincinnati	17.7	15.6	10.7	7.7	12.9	10.6	50.7	44.0	33.7
Baltimore	13.3	13.7	11.4	9.2	11.9	17.7	62.0	54.0	49.8
Kansas City	11.1	10.1	7.9	6.1	8.8	9.6	60.7	54.4	28.6
Indianapolis	10.0	8.4	5.4	3.8	6.9	12.1	50.2	43.9	27.1

[a] Foreign-born; persons of mixed or foreign-born parentage; and non-white persons.

non-white in 1930; and the mean Democratic percentage of the presidential vote, 1928–40.

Inspection of the table suggests that percentage foreign-born, or of foreign derivation, *was* associated in some degree with Democratic presidential voting. In statistical terms, the association is strengthened when the mean foreign-born white percentage, 1900–30, is combined with the percentage non-white in 1930; but the correlation is still not especially strong.[27] Statistics apart, the association is probably strengthened by Samuel Lubell's argument that higher birth rates prevailed, in that period, among immigrant families and urban low-income groups than among native and upper-income groups. In considering the correlation coefficient, one might also allow for North European (generally non-Democratically disposed) immigration in Minneapolis, Milwaukee, and the Pacific Northwest, and for the delaying effects of local leadership and political organization on Negro party identification in such cities as Cleveland, Philadelphia, and St. Louis.[28] Today it is often argued that recent metropolitan migration patterns have probably reduced politically relevant demographic diversity and thus dimin-

[27] The coefficient of correlation between columns II and IV is .44; that between the sum of columns II and III, on the one hand, and column IV, on the other, is .51. The coefficient of correlation between column II and the percentage non-white population *in 1950*, on the one hand, and the average Democratic percentage of the presidential vote, *1944–60*, is .65; but that between column II alone and the presidential percentage, 1944–60, is only .18.

[28] See Lubell, Chaps. 3 and 5, and the data on urban birth rates in W. Thompson and P. K. Whelpton, *Population Trends in the U. S.* (New York, 1933), Chap. 8. In Cleveland the predominantly Negro wards remained Republican throughout the nineteen-thirties, despite serious Democratic efforts to reverse their allegiance and recurrent pre-election rumors that a switch was coming. The Republican organization was strongly based on these wards, which finally went Democratic in 1943 (the second election of Mayor Lausche). In Philadelphia many Negroes changed their vote to Democratic in the early thirties and returned to the Republican fold in the election of 1939. (See especially *The New York Times* for November 9, 1939.) In St. Louis the Republican Negro vote was wavering for local reasons in the late twenties, the Negro wards went decisively Democratic in 1933, shifted back to the Republicans (again, largely for local reasons) in 1941, and went Democratic again in 1949 and subsequently; that is, the "Negro vote" was with the majority on each occasion and was clearly an important influence. Insufficient recognition by patronage is said to have been one important local factor in the shift against the Democrats in 1941. See especially *St. Louis Post-Dispatch*, April 2, 1941, and the fictional treatment by Fanny Cook, *Mrs. Palmer's Honey* (New York, 1945).

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ished electoral competition in many large cities.[29] Striking increases have occurred in many large cities in the non-white proportion of the population; and the "ethnic" or "foreign stock" component has more than held its own, though it is hard to measure beyond the second generation. This is the most dramatic change, for measures of population diversity or concentration in the lower ranges of education, occupation, and income show relatively little change over the period.[30] On these indicators of class and status, big city populations have not increased in homogeneity; but "minority group" members are typically the majority in large cities today and, to date, have strongly tended to be Democratic in their party identification.[31]

A second aspect of this history suggests that our general proposition must be limited to the long run. In the instances where Democratic electoral dominance was *quickly* achieved in local elections, corruption had discredited Republican machines and/or minority Democratic organizations were already rather strong. The Depression and the political and programmatic efforts of the New Deal severely shook most big city party organizations, Republican and Democratic; and some Republican organizations recovered in the nineteen-thirties while some Democratic organizations did not. Thus, the character of local political organization was of some independent importance in the short run.

It may be useful to distinguish between electoral dominance and organization control, the former resting on party identification, the latter on techniques ranging from social

29 See Lubell; and E. C. Banfield, "The Politics of Metropolitan Integration," *Midwest Journal of Political Science,* I (1957), 77, and references there cited, for related discussions.

30 The data are not reproduced here. Census classifications of occupations and education were scaled and means and standard deviations were computed as well as proportions in the lower classifications. For income, the semi-interquartile range was computed as well as proportions below and above certain arbitrarily chosen extremes. The general conclusion must be that, by these social-economic indicators, there has been little change in diversity within the big city populations; but the increase in non-white and "ethnic" has been pronounced. "Ethnic" here is the Census usage: the foreign-born and those of foreign-born or mixed parentage.

31 For the twenty cities considered here the coefficient of correlation between percentage of population "ethnic" and non-white, on the one hand, and the mean Democratic percentage of the presidential vote, 1956–60, on the other hand, is .5986.

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and economic sanctions to the *quid pro quo* to suasion based upon purely personal influence in the precincts. The distinction is not as clear practically as it is conceptually, for many votes must be determined by some combination of electoral dominance and organizational control, and each could be seen as reinforcing the other. Few city political organizations have escaped recurrent factionalism or ineffectiveness. Nonetheless, the distinction between electoral dominance and organization control does lead to two important points. (1) In the cities under examination here Republican organizations have ultimately lost local control after continued Democratic presidential majorities (even though their own atrophy or excess immediately accounted for their downfall). (2) Some cities display marked Democratic electoral dominance in the absence of strong organization control, but the reverse is not the case. Together, these points seem further to support the proposition that the influence of national politics was decisive in the long run. They are further examined in Section III below.

II

What has already been said about the long-run relations between presidential and local voting indicates that the short-run relation will not normally be a close one. There are two illustrations of this in the history reviewed above. One is that, in the late thirties or early forties, several large cities experienced Republican recoveries, notably Buffalo, Indianapolis, and St. Louis. In Pittsburgh the Republicans briefly challenged the Democrats before finally succumbing; in Philadelphia the Democratic threat of the nineteen-thirties subsided; in Kansas City the Democrats lost to the forces of reform allied with Republicans; and in Baltimore the Republicans elected a mayor. This occurred in a period of decline in Democratic presidential and congressional margins from their 1934–36 highs, but local conditions seem also to have been important in most cases. Local conditions were evidently of great importance in St. Louis, especially the exposure of Democratic organizational excesses and the reversal of Negro voting noted above. In Philadelphia, local and national electoral trends moved in opposite directions. The local conditions, including formal nonpartisanship, affecting

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Kansas City are well known. Democratic factionalism played an important role in Pittsburgh and Baltimore, and in Baltimore the Democratic reversal was confined to a controversial four-term mayor, while the City Council was unaffected. Trends in party preferences related to national politics must certainly have assisted these local Republican revivals, but it seems doubtful that this alone was controlling. Buffalo and Indianapolis displayed no dramatic short-run change since neither city had ceased to be competitive nationally or locally, and in Buffalo the Democratic party was chronically plagued with factionalism and by minor parties.

A much more striking instance of local electoral independence is that of the Eisenhower elections of the nineteen-fifties and, more broadly, the entire postwar period. In this period Democratic electoral dominance of most large cities was remarkably stable in local elections. Reference to Table 1 will reveal the short-run independence of local and presidential elections. While the Democratic presidential trend is generally downward through the 1956 election and then is sharply reversed in 1960, there is no corresponding trend in mayoral elections in the partisan cities. In those cities that might be said to have "lagged" in converting to Democratic dominance locally (Philadelphia, St. Louis, and, less markedly, Buffalo and Indianapolis) the Democratic trend in mayoral elections is generally upward throughout the period; but in the other partisan cities there is no discernible trend or pattern. In general the mayoral vote is somewhat more variable than the presidential vote, though the variances of the two sets of series do not differ systematically. Democratic mayoral margins are not consistently higher or lower than presidential margins, but there was a tendency toward the end of the period for the local Democratic margins to exceed the presidential as the the latter fell off in the Eisenhower elections. All this suggests that local elections in the partisan cities are, within the limits resulting from long-run party identifications, quite independent of short-run national changes. To put the same point another way: short-run national trends have evidently not disturbed the long-run bases of local Democratic dominance.[32]

[32] See Angus Campbell, *et al.*, *The American Voter* (New York, 1960), Chap.

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Two related factors help explain the short-run local independence. One is the fact that all of the partisan large cities hold local elections in non-presidential years.[33] Although ticket-splitting was common in the Eisenhower elections, it seems probable that the staggering of elections aided the local Democrats.[34] Similarly, the lower turnout in local elections may have favored the Democratic "organization" vote over the Republican vote since, even in the weakly organized cities, the dominant Democratic party is usually better organized than is the Republican minority.[35]

There is, in fact, a bare indication in the election data that party organization makes a difference in local turnout, though the case is far from conclusive. Table 3 shows turnout data for large cities classified as partisan, doubtful, and nonpartisan (the "doubtful" cities are formally nonpartisan, but party lines are salient in local elections). No cities are included that hold municipal elections concurrently with contests for "higher" jurisdictions, and the one-party cities of the South are omitted. The turnout figures are means for mayoral elections over the postwar period. It is clear from the table that factors other than partisanship-nonpartisanship are at work; but there is some tendency for turnout in partisan cities to exceed that in nonpartisan cities, and to be highest in those partisan cities characterized by tight party organization control, of which control of primaries is the principal test.[36] The tendency is less marked in presidential elections,

6 and the same authors' "Stability and Change in 1960: A Reinstating Election," *American Political Science Review,* LX (1961), 273–74, for evidence that the distribution of party identifications in the country at large did not change much during the Eisenhower administrations. It seems reasonable to assume, in light of the Survey Research Center data, that this was true for the population of large cities apart from demographic change.

33 This is true of most large cities and of all the partisan cities in Table 1. Indeed, each of these cities *now holds* municipal elections in odd-numbered years, free of the immediate influences of both state and national elections.

34 On ticket-splitting in the Eisenhower elections, see Campbell, *et al., The American Voter.* On the "insulating" effect on state politics of off-year state elections, see V. O. Key, *American State Politics* (New York, 1956), Chap. 2.

35 Here, and for purposes of the discussion following, turnout is computed as a percentage of the population twenty-one years of age and older, rather than as a percentage of registered voters. For inter-censal years some interpolations are involved.

36 Buffalo is the chief exception, in terms of party organization, in Table 3; but elections have been competitive there. Use of medians rather than means does not significantly change the ranking in Table 3.

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TABLE 3

SELECTED PARTISAN AND NONPARTISAN CITIES RANKED BY MEAN ELECTION TURNOUTS IN MAYORAL ELECTIONS, 1944–1960

Partisan	Doubtful	Nonpartisan
Chicago (51.5)		
Pittsburgh (50.5)		
Philadelphia (49.8)		
Buffalo (49.1)		
	Cincinnati (49.0)	
		Boston (47.0)
		San Francisco (46.3)
New York (42.3)		
Indianapolis (41.4)		
		Seattle (39.8)
		Minneapolis (37.0)
		Denver (36.7)
	Cleveland (34.8)	
		Detroit (33.8)
Baltimore (31.5)		
		Los Angeles (31.3)
St. Louis (30.4)		
	Kansas City (29.9)	

and it appears from Table 3 that some broadly regional factors are also at work. The differences between the two groups of partisan and nonpartisan cities are not statistically significant.[37] The turnout data are not very reliable but it does appear that partisanship marginally increases turnout in local elections. This, in turn, may indicate that if an "organization" vote exists—and if the majority party may be assumed to enjoy organizational superiority—then off-year elections favor the dominant local party.

This tenuous line of argument is not, however, necessary to the main point: that, in the short run, local elections in the partisan cities show some independence of presidential trends to the recent advantage of the dominant Democratic party. Off-year elections and majority organizational superiority (which varies greatly among the cities) are probably part of the explanation, but it seems likely that persisting

[37] The test was made in terms of Student's "t" at the five per cent level. If means for all mayoral *elections* in the two groups, rather than individual city means, are used the difference is significant, though not dramatically so, at the one per cent level: T = 3.2; N (partisan elections) = 32; Np (nonpartisan elections) = 39. This is so although there is no difference in mean electoral margins of the two groups, so that close elections are evidently not influencing the result.

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party identifications, more stable respecting local than national elections, are at least of equal if not of isolable importance.

III

The question discussed in the preceding section can be turned around: How important are big city political organizations in presidential elections? It is often alleged either that city "machines" deliver the presidential vote, or, less commonly, that they depend upon it for local control. The role of our large cities in presidential elections was explored by Eldersveld (see n.5 above) in 1949 and no attempt is made here to extend that analysis. Instead, some suggestions in the available data are brought to bear on the question of the importance of city political "machines" or organizations in presidential elections.

The partisan large cities appear to vary considerably as respects the cohesiveness and effectiveness of their party organizations. By cohesiveness is meant freedom from tendencies to factionalism, either regional or city-wide; and by effectiveness is meant the organization's demonstrated ability to win primaries and its *reputed* effectiveness in general elections as reported by newspapers and political scientists. Three broad types of party structure can be defined: those exhibiting high cohesiveness and effectiveness; those in which the majority party contains two or more continuing factions, each of which may be relatively "effective" in its own bailiwick; and those that are weakly organized and reported to be relatively ineffective at the ward or precinct level. Of the cities under discussion here, those that are most party-competitive do not have party organizations of the first type; but neither do all of the most dominantly Democratic cities. It thus appears that differences in cohesiveness and effectiveness of local party organization are not systematically associated with the size of Democratic presidential pluralities or with the degree of *local* Democratic electoral dominance; in most cities these outcomes appear primarily to depend upon cultural and socio-economic determinants of party identification. This general conclusion may be defended and detailed in several ways:

(1) Of the *nonpartisan* large cities, two, Boston and De-

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troit, have been as Democratic in presidential elections as any of the *partisan* large cities.[38] Another nonpartisan city, Milwaukee, has regularly produced Democratic presidential margins close to those of its neighbor, the strongly organized city of Chicago. These cities differ in degree and nature of nonpartisanship; local elections in Detroit are normally quite effectively nonpartisan; those in Milwaukee are somewhat less so, in the sense that candidates have had partisan identities though the party organizations have not openly intervened in elections; and in Boston nearly all candidates are Democrats and have sometimes represented party factions, though the Democratic party has recently tried to maintain its own neutrality among the contenders.[39] Each of these cities is reported to have an effective Democratic party organization for state and national elections, but none of these organizations depends on *local* patronage, and those in Detroit and Milwaukee have little in common with the classical urban "machine." Local control, then, is evidently not requisite to electoral success in presidential elections, at least in cities where *neither* side can accomplish control.

(2) In fact, the variance in the Democratic percentage of the presidential vote, 1944–60, has been no greater in the nonpartisan than in the partisan large cities; but it is true that several nonpartisan cities west of the Mississippi have been more Republican in presidential elections than have any of the partisan cities. From this one *might* conclude that party organization served to reinforce and maintain Democratic voter predispositions, but this conclusion would ignore the marked sectionalism evident in city voting in presidential elections since, say, 1944. It is generally true that the more effectively nonpartisan cities are located west of the Mississippi, and that degree of party organization and degree of Democratic presidential voting in cities appear to

[38] Generally in the postwar period the large cities west of the Mississippi have been closely competitive and frequently Republican in presidential elections. These are *all* nonpartisan cities, and the difference seems to lie largely in regional and demographic factors rather than in partisan *versus* nonpartisan elections.

[39] See the reports on these three cities in the series of *City Politics Reports*, edited by E. C. Banfield (mimeographed, Joint Center for Urban Studies of Harvard University and M.I.T. [Cambridge, Mass., 1960 and 1961]). The Democratic registration majority is large in each city, but especially so in Boston, where the margin is reported to be 7:1.

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be associated; but the direction of the relationship is not at all clear, in view of what has been said above; and the association may even be spurious.

(3) Two of the large cities (Cleveland and St. Louis) where party organization is *relatively* weak—at least on the city-wide level—show very wide mean Democratic margins in Table 2 and have lately been heavily dominated by the Democratic party in local elections.[40] This indicates that Democratic dominance in either presidential elections *or* local elections does not necessarily depend upon highly cohesive and effective local organization.

(4) Variability in the presidential vote has been as great (in some cases greater) in the reputedly strongly organized cities as in cities with organizations low in cohesiveness or reported effectiveness. The variance in Democratic presidential margins, 1936–60, shows little relation to cohesiveness or reported effectiveness.[41] For instance, both Baltimore and Chicago are "safely" Democratic cities locally, as we saw in Section I; yet in neither city was the Democratic organization able to deliver a Democratic presidential majority in the election of 1956. Cleveland and St. Louis (presumably "weakly" organized cities) did deliver Democratic majorities, but they started with a higher "normal" Democratic vote. Democratic presidential majorities fell sharply in the Eisenhower years in New York and Pittsburgh; only in Philadelphia, among the reputedly well organized cities, was the variance in presidential margins minimal.[42] The 1960 election affords another interesting study; the percentage increase in the Democratic presidential margin over 1956 bears little

40 There have been close primary election contests in both cities in the postwar period, indicating low organization effectiveness; but Republican candidates have not seriously threatened in Cleveland since 1943 and St. Louis since 1949. Party organization seems to have been declining in effectiveness in St. Louis in recent years. The possibility that Cleveland's nonpartisan primary weakens organization control was noted above.

41 For various reasons, the period 1936–60 seems the fairest to use. The variances in margins were calculated and related by simple inspection to the reports of party "strength" taken from newspapers and such scholarly literature as was available and to organization performances in primary elections.

42 Attention should be called to the sharp decline in Democratic presidential margins in New York and Philadelphia in 1948 (see Table 1), evidently the result of the third party vote, and indicating low organizational effectiveness on that occasion.

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relation to reported organization strength in the several partisan cities in Table 1.

(5) It may be that big city political organizations, even if insignificant as independent determinants of party preference, do substantially influence presidential elections by increasing the turnout, if only marginally, of those already disposed to vote Democratic. On this point, too, the evidence is mixed; but there is some support for the proposition. The available data on presidential turnout and in Table 3 above indicate that: (a) Turnout in *local* elections tends to be higher in the partisan cities than in the nonpartisan cities, but there is a strong suggestion that regional differences are involved. (b) Turnout in *partisan* local elections tends to be highest in those cities—Chicago, Philadelphia, and Pittsburgh—whose party organizations score highest on cohesiveness and effectiveness by reputation and primary election performance. (c) Among the partisan cities these same three cities stand at or close to the top in turnout in *presidential* elections (in 1948, 1952, and 1960); but so do some partisan cities—such as Buffalo—much lower in party cohesiveness. (d) Moreover, several nonpartisan cities score as high or higher in presidential election turnout as any of the partisan cities; other things equal, partisanship is not significantly related to turnout in presidential elections. (e) The more cohesive and effective city organizations (by other tests) appear to do somewhat better in *registration* than most other cities, but the data on this point are not very good.[43] It appears from all this that certain city organizations—particularly in Chicago, and probably in Philadelphia and Pittsburgh—have some effect upon turnout. They are the city organizations that stand highest in cohesiveness and effectiveness by other indications. They are located in states that, of all the parent states of the cities examined here, have consistently had the closest electoral margins in state-wide elections since 1944.[44] This, taken with the prestige and

[43] For one thing, it is necessary to use registration *in all parties* as the datum.

[44] Means and standard deviations of contests for President and Governor were examined, and Illinois and Pennsylvania scored lowest. *State* turnout (total state vote as percentage of the state population twenty-one and older) in those states does not seem to differ systematically from that in the other states containing large cities.

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patronage at stake, may provide both incentive and wherewithal for high organizational effectiveness and thus afford a partial explanation of it.

More generally, it could be argued that party organization enlarges turnout because the partisan cities are those with the larger low-status, low-income populations that are least likely to vote; that the partisan cities hold their own in presidential turnout with nonpartisan cities is thus evidence of party effectiveness. One could add that nonpartisan cities like Boston with high turnout have effective party organization for other than local elections and relatively ineffective nonpartisan local traditions. Table 4 indicates that the partisan cities tend to have higher proportions of low-income and low-status population than have the nonpartisan cities, while equaling nonpartisan cities in presidential turnout and surpassing them in local elections. As in several analyses above, regional factors of some sort are obviously at work; for, except for San Francisco in respect to turnout, the nonpartisan cities in the minority rows are always and only those east of

TABLE 4

SOCIO-ECONOMIC FACTORS AND TURNOUT IN PARTISAN AND NONPARTISAN LARGE CITIES

	Per Cent of Population in White Collar Jobs	Median Family Income	Per Cent Family Incomes <$3,000	Per Cent Family Incomes >$10,000	Median Years of School Completed	Local Election Turnout	1960 Presidential Turnout
Partisan Cities Above Median	2	3	6	2	1	6	5
Partisan Cities Below Median	7	6	3	7	8	3	4
Nonpartisan Cities Above Median	7	7	2	8	8	2	5
Nonpartisan Cities Below Median	2	2	7	1	1	5	3

Note: Cincinnati and Kansas City were included in evaluation of medians but not in table. Cleveland was classified "partisan." Based on Tables 32 and 33 of the 1960 Census, *General Social and Economic Characteristics.*

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the Mississippi. Nevertheless, it seems a fair conclusion from Table 4 that the partisan cities have higher electoral turnout than would be expected if their elections were held under nonpartisan conditions.

IV

The conclusions and suggestions that emerge from this account and analysis are not novel, but perhaps they will extend our understanding of the relations between national politics and big city politics. They can be briefly summarized:

(1) There was a secular trend in city Democratic voting, both national and local, underway in many cities in the nineteen-twenties, as has often been pointed out. Some of the issues involved—chiefly respecting the position of urban "minorities"—were "nationalized" by the New Deal; they became subjects of national policy and political action. This is probably a major reason why national politics are most important in the party preferences that now heavily influence city politics, although the trend to Democratic voting was in some cities as evident in local as in national elections in the nineteen-twenties. Historically the movement began with city politics at least as much as with national politics; in particular, ethnic and religious rivalries were important in several cities in the nineteen-twenties and these rivalries, as in Baltimore and Indianapolis, occasionally gave rise to bitter interparty cleavage in city campaigns and elections.

(2) The effect of the Depression on city politics was profound. Dissatisfaction with governmental performance quickly became widespread and publicly accepted, and this probably accelerated the group tensions already undermining urban political "machines" in some cities—Baltimore and New York in particular, among the largest cities. In some cities strong organizations survived or were created, but few large city organizations today avoid serious factional challenges.

(3) Scandal and corruption often have significant effects on city elections. In most instances discussed above, exposure of official corruption was abetted in its effect by Democratic national election trends, but in St. Louis and Kansas City in the nineteen-forties city scandals probably

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played an important role in political change. This would indicate that the minority party does have a role to play as a "gun behind the door" even where party competition is not normally close.

(4) Close and regular party competition in city elections is rare, and Democratic dominance is characteristic; but several Democratic organizations have lost city-wide elections—either primary or general—in the postwar period. There is great variety in organizational effectiveness, dependent on a variety of factors including patronage, electoral systems, social or regional factionalism, the size, distribution, and leadership of the low social rank population, and the activity and alignment of city elites, including the newspapers.

(5) Party preferences related to national politics appear to be controlling in city elections in the long run. This suggests that, other things being equal, it would take a change in group alignments respecting national politics in most large partisan cities to put the Republicans in a regularly competitive position locally.

(6) *In the short run,* city elections have not been seriously affected by changes in voter orientations to national elections for several reasons suggested in Section II above. But national politics can have an effect in the few closely competitive large cities, just as most factors are more effective under these conditions. In the long run, party organization is probably an important factor in hastening or delaying local response to national trends; a powerful organization, especially when it is undamaged by scandal, may tend to discourage minority voting simply because it seems futile; where patronage is freely available it may capture parts of the minority organization. But persistent presidential majorities seem to be controlling.

(7) There is no reason adduced above to doubt that city political organization is important in state and national elections by influencing turnout and marginally offsetting "outstate" results. Organizations tend to make the most of existing partisan predispositions. But in the long run, party preferences rather than organization control seem to be decisive in both city and national elections, and it seems doubtful from the analysis above that these preferences are primarily determined by organization dominance or control.

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(8) The cities under discussion here are located outside what are commonly thought of as the South and West. That is because, in those regions, most cities are nonpartisan in law and/or in fact. In most nonpartisan cities it is especially unlikely that local "machines" are seriously affecting the presidential or local vote. There is, however, a significant regional difference in big city voting in postwar presidential elections; Republicans have done better in the West (including Cincinnati and Indianapolis) than in the East. In the light of the argument so far it seems unlikely that partisanship-nonpartisanship accounts for the difference. The demographic data in Table 2 above, together with occupational and home ownership data indicating somewhat smaller low social rank populations in several Western cities, suggest that variations in urban populations help account for the political differences, and so perhaps do regional variations in political cultures or attitudes. It hardly seems that restricting the discussion broadly to the Northeast results in understating the importance of city political organizations.

(9) These conclusions imply skepticism about the influence of city political machines today and in the recent past in presidential elections—or, in the long run, in local elections. But they also indicate real variation in organizational effectiveness, which can best be understood by observation in depth and detail rather than, as here, in some breadth and perspective.

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A STUDY IN NATIVISM: THE AMERICAN RED SCARE OF 1919–20*

AT A victory loan pageant in the District of Columbia on May 6, 1919, a man refused to rise for the playing of "The Star-Spangled Banner." As soon as the national anthem was completed an enraged sailor fired three shots into the unpatriotic spectator's back. When the man fell, the *Washington Post* reported, "the crowd burst into cheering and handclapping." In February of the same year, a jury in Hammond, Indiana, took two minutes to acquit the assassin of an alien who yelled, "To Hell with the United States." Early in 1920, a clothing store salesman in Waterbury, Connecticut, was sentenced to six months in jail for having remarked to a customer that Lenin was "the brainiest," or "one of the brainiest" of the world's political leaders.[1] Dramatic episodes like these, or the better known Centralia Massacre, Palmer Raids, or May Day riots, were not everyday occurrences, even at the height of the Red Scare. But the fanatical one hundred per cent Americanism reflected by the Washington crowd, the Hammond jury, and the Waterbury judge pervaded a large part of our society between early 1919 and mid-1920.

* I am grateful to Robert D. Cross and Clyde C. Griffen for their critical reading of this article, and to Anthony F. C. Wallace and Abram Kardiner for their helpful comments on my use of anthropological and psychological material.

1 *Washington Post,* May 7, 1919; Mark Sullivan, *Our Times, The United States 1900–1925* (New York, 1935), VI, 169; *The Nation,* CX (April 17, 1920), 510–11. The most complete account of the Red Scare is Robert K. Murray, *Red Scare, A Study in National Hysteria* (Minneapolis, 1955). But see the critical review of Murray's book by John M. Blum in *Mississippi Valley Historical Review,* XLII (1955), 145. Blum comments that Murray failed to explain "the susceptibility of the American people and of their elite to the 'national hysteria.' . . . About hysteria, after all, psychology and social psychology in particular have had considerable to say." John Higham places the postwar movement in historical perspective in his superb *Strangers in the Land, Patterns of American Nativism, 1860–1925* (New Brunswick, 1955), especially Chaps. 8 and 9.

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Recently, social scientists have produced illuminating evidence about the causes of eruptions like that of 1919–20. They have attempted to identify experimentally the individuals most responsive to nativistic appeals, to explain their susceptibility, and to propose general theories of nativistic and related movements. These studies suggest a fuller, more coherent picture of nativistic upheavals and their causes than we now possess, and they provide the framework for this attempt to reinterpret the Red Scare.

Psychological experiments indicate that a great many Americans—at least several million—are always ready to participate in a "red scare." These people permanently hold attitudes which characterized the nativists of 1919–20: hostility toward certain minority groups, especially radicals and recent immigrants, fanatical patriotism, and a belief that internal enemies seriously threaten national security.[2]

In one of the most comprehensive of these experiments, psychologists Nancy C. Morse and Floyd H. Allport tested seven hypotheses about the causes of prejudice and found that one, national involvement or patriotism, proved to be "by far the most important factor" associated with prejudice. Other widely held theories about prejudice—status rivalry, frustration-aggression, and scapegoat hypotheses, for example —were found to be of only secondary importance.[3] Summarizing the results of this and a number of other psychological experiments, Gordon W. Allport, a pioneer in the scientific

[2] On the incidence of prejudice against minorities in the United States, see Gordon W. Allport and Bernard M. Kramer, "Some Roots of Prejudice," *Journal of Psychology*, XXII (1946), 9–39; Morris Janowitz and Dwaine Marvick, "Authoritarianism and Political Behavior," *Public Opinion Quarterly*, XVII (1953), 185–201; Bruno Bettelheim and Morris Janowitz, *Dynamics of Prejudice, A Psychological and Sociological Study of Veterans* (New York, 1950), 16, 26, and *passim*.

[3] Nancy C. Morse and F. H. Allport, "The Causation of Anti-Semitism: An Investigation of Seven Hypotheses," *Journal of Psychology*, XXXIV (1952), 197–233. For further experimental evidence indicating that prejudiced individuals are no more anxious, neurotic, or intolerant of ambiguity than those with more "liberal" attitudes, Anthony Davids, "Some Personality and Intellectual Correlates to Intolerance of Ambiguity," *Journal of Abnormal and Social Psychology*, LI (1955), 415–20; Ross Stagner and Clyde S. Congdon, "Another Failure to Demonstrate Displacement of Aggression," *Journal of Abnormal and Social Psychology*, LI (1955), 695–96; Dean Peabody, "Attitude Content and Agreement Set in Scales of Authoritarianism, Dogmatism, Anti-Semitism and Economic Conservatism," *Journal of Abnormal and Social Psychology*, LXIII (1961), 1–11.

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study of prejudice, concluded that in a large proportion of cases the prejudiced person is attempting to defend himself against severe inner turmoil by enforcing order in his external life. Any disturbance in the social *status quo* threatens the precarious psychic equilibrium of this type of individual, who, according to Allport, seeks "an island of institutional safety and security. The nation is the island he selects. . . . It has the definiteness he needs."

Allport pointed out that many apprehensive and frustrated people are not especially prejudiced. What is important, he found,

> is the way fear and frustration are handled. The institutionalistic way—especially the nationalistic—seems to be the nub of the matter. What happens is that the prejudiced person defines 'nation' to fit his needs. The nation is first of all a protection (the chief protection) of him as an individual. It is his in-group. He sees no contradiction in ruling out of its beneficent orbit those whom he regards as threatening intruders and enemies (namely, American minorities). What is more, the nation stands for the status quo. It is a conservative agent; within it are all the devices for safe living that he approves. His nationalism is a form of conservatism.[4]

Substantial evidence, then, suggests that millions of Americans are both extraordinarily fearful of social change and prejudiced against those minority groups which they perceive as "threatening intruders." Societal disruption, especially if it can easily be connected with the "intruders," not only will intensify the hostility of highly prejudiced individuals, but also will provoke many others, whose antagonism in more stable times had been mild or incipient, into the extreme group.

A number of anthropologists have come to conclusions about the roots of nativism which complement these psychological studies. Since the late nineteenth century, anthropologists have been studying the religious and nativistic cults of American Indian tribes and of Melanesian and Papuan groups in the South Pacific. Recently, several anthropologists have attempted to synthesize their findings and

[4] Gordon W. Allport, *The Nature of Prejudice* (Cambridge, 1955), 406; see Boyd C. Shafer, *Nationalism, Myth and Reality* (New York, 1955), 181.

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have shown striking parallels in the cultural conditions out of which these movements arose.[5] In every case, severe societal disruption preceded the outbreak of widespread nativistic cult behavior. According to Anthony F. C. Wallace, who has gone farthest toward constructing a general theory of cult formation, when the disruption has proceeded so far that many members of a society find it difficult or impossible to fulfill their physical and psychological needs, or to relieve severe anxiety through the ordinary culturally approved methods, the society will be susceptible to what Wallace has termed a "revitalization movement." This is a convulsive attempt to change or revivify important cultural beliefs and values, and frequently to eliminate alien influences. Such movements promise and often provide participants with better means of dealing with their changed circumstances, thus reducing their very high level of internal stress.[6]

[5] See, especially, the works of Anthony F. C. Wallace: "Revitalization Movements," *American Anthropologist,* LVIII (1956), 264–81; "Handsome Lake and the Great Revival in the West," *American Quarterly,* IV (1952), 149–65; "Stress and Rapid Personality Change," *International Record of Medicine and General Practice Clinics,* CLXIX (1956), 761–73; "New Religions Among the Delaware Indians, 1600–1900," *Southwest Journal of Anthropology,* XII (1956), 1–21. Also, Michael M. Ames, "Reaction to Stress: A Comparative Study of Nativism," *Davidson Journal of Anthropology,* III (1957), 16–30; C. S. Belshaw, "The Significance of Modern Cults in Melanesian Development," *Australian Outlook,* IV (1950), 116–25; Raymond Firth, "The Theory of 'Cargo' Cults: A Note on Tikopia," *Man,* LV (1955), 130–32; Lawrence Krader, "A Nativistic Movement in Western Siberia," *American Anthropologist,* LVIII (1956), 282–92; Ralph Linton, "Nativistic Movements," *American Anthropologist,* XLV (1943), 220–43; Margaret Mead, *New Lives for Old* (New York, 1956); Peter Worsley, *The Trumpet Shall Sound* (London, 1957). Several sociologists and psychologists have come to conclusions about the causes of these movements that are similar in important respects to Wallace's, although less comprehensive. See Leon Festinger, *A Theory of Cognitive Dissonance* (New York, 1957); Hadley Cantril, *The Psychology of Social Movements* (New York, 1941), especially pp. 3–4, Chaps. 5, 8, and 9; Hans H. Toch, "Crisis Situations and Ideological Revaluation," *Public Opinion Quarterly,* XVIX (1955), 53–67.

[6] Wallace, "Revitalization Movements." For a recent verification of Wallace's theories see Thomas Rhys Williams, "The Form of a North Borneo Nativistic Behavior," *American Anthropologist,* LXV (1963), 543–51. On the psychological results of socially caused stress, Wallace, "Stress and Rapid Personality Change"; William Caudill, *Effects of Social and Cultural Systems in Reactions to Stress,* Social Science Research Council Pamphlet No. 14 (New York, 1958); Caudill, "Cultural Perspectives on Stress," Army Medical Service Graduate School, *Symposium on Stress* (Washington, D. C., 1953); Hans Selye, *The Stress of Life* (New York, 1956); Roland Fischer and Neil Agnew, "A Hierarchy of Stressors," *Journal of Mental Science,* CI (1955), 383–86; Daniel

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American Indian tribes, for example, experienced a series of such convulsions as the tide of white settlers rolled west. The Indians were pushed onto reservations and provided with Indian agents, missionaries, and physicians, who took over many of the functions hitherto assumed by chiefs and medicine men. Indian craftsmen (and craftswomen) were replaced by dealers in the white man's implements. Most hunters and warriors also lost their vocations and consequently their self-respect. What an anthropologist wrote of one tribe was true of many others: "From cultural maturity as Pawnees they were reduced to cultural infancy as civilized men." [7]

One of the last major religious upheavals among the Indians was the Ghost Dance cult which spread from Nevada through Oregon and northern California in the eighteen-seventies, and a similar movement among the Rocky Mountain and western plains Indians about 1890. Although cult beliefs varied somewhat from tribe to tribe, converts generally were persuaded that if they followed certain prescribed rituals, including the dance, they would soon return to their old ways of living. Even their dead relatives would be restored to life. Most Indians were too conscious of their military weakness to challenge their white masters directly. Ghost Dancers among the Dakota Sioux, however, influenced by the militant proselyter Sitting Bull, became convinced that true believers could not be harmed by the white man's bullets and that Sioux warriors would drive the intruders from Indian lands. Their dreams were rudely smashed at the massacre of Wounded Knee Creek in December 1890.[8]

H. Funkenstein, Stanley H. King, and Margaret E. Drolette, *Mastery of Stress* (Cambridge, 1957); M. Basowitz *et al.*, *Anxiety and Stress: An Interdisciplinary Study of a Life Situation* (New York, 1955).

[7] Alexander Lesser, *The Pawnee Ghost Dance Hand Game. A Study of Cultural Change* (New York, 1933), 44.

[8] Cora DuBois, *The 1870 Ghost Dance*, Anthropological Records, III (Berkeley, 1946); Leslie Spier, *The Ghost Dance of 1870 Among the Klamath of Oregon*, University of Washington Publications in Anthropology, II (Seattle, 1927); Lesser, *Ghost Dance;* A. L. Kroeber, *Handbook of the Indians of California*, Bureau of American Ethnology Bulletin 78 (Washington, D. C., 1925). Anthropologists recently have argued about the origins of the Ghost Dance cults. Both sides agree, however, that whatever their origins, the cults took the form they did because of intolerable cultural conditions caused largely by white encroachments. David F. Aberle, "The Prophet Dance and Reactions to White Contact," *Southwest Journal of Anthropology*, XV (1959), 74–83;

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The Boxer movement in China, 1898 to 1900, resembled in many respects the Indian Ghost Dance cults; however, the Boxers, more numerous and perhaps less demoralized than the Indians, aimed more directly at removing foreign influences from their land. The movement erupted first in Shantung province where foreigners, especially Japanese, British, and Germans, were most aggressive. A flood of the Yellow River had recently deprived about a million people in the province of food and shelter. Banditry was rampant, organized government ineffective. The Boxer movement, based on the belief that these tragic conditions were due almost entirely to the "foreign devils" and their agents, determined to drive the enemy out of China. Boxers went into action carrying charms and chanting incantations supposed to make them invulnerable to the foreigners' bullets. The first object of the Boxers' nativistic fury were Chinese who had converted to Christianity, the intruders' religion. The patriots then attacked railroad and telegraph lines, leading symbols of foreign influence. Finally, the Boxers turned against the foreigners themselves, slaughtering many. Not until after the Boxers carried on a two-month siege of the foreign community in Peking did American, European, and Japanese armies crush the movement.[9]

Other revitalization attempts proved more successful than the Boxers or Ghost Dancers. The Gaiwiio movement, for example, helped the Iroquois Indians of western New York State to retain their identity as a culture while adjusting successfully to an encroaching white civilization during the first decade of the nineteenth century. The movement implanted a new moral code among the Indians, enjoining sobriety and family stability and encouraging acceptance of Western technology, while revivifying cohesive Indian traditions.[10]

Leslie Spier, Wayne Suttles, and Melville Herskovits, "Comment on Aberle's Thesis of Deprivation," *Southwest Journal of Anthropology*, XV (1959), 84–88.

9 The best account of the Boxer movement is Chester C. Tan, *The Boxer Catastrophe* (New York, 1955). Also, George N. Steiger, *China and the Occident, the Origin and Development of the Boxer Movement* (New Haven, 1927); Peter Fleming, *The Siege at Peking* (New York, 1959).

10 Wallace, "Handsome Lake." Wallace compared the Gaiwiio with a Chinese attempt to accommodate their society to Western civilization in "Stress and Rapid Personality Change." For a successful movement in the South Pacific see Mead, *New Lives for Old.*

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Dominant as well as conquered peoples, Ralph Linton has pointed out, undergo nativistic movements. Dominant groups, he observed, are sometimes threatened "not only by foreign invasion or domestic revolt but also by the invidious process of assimilation which might, in the long run, destroy their distinctive powers and privileges." Under such circumstances, Linton concluded, "the frustrations which motivate nativistic movements in inferior or dominated groups" are "replaced by anxieties which produce very much the same [nativistic] result" in dominant groups.[11]

Communist "brainwashers" have consciously attempted to achieve results comparable to those obtained by prophets of movements like the Ghost Dance cult and the Boxers. They create intolerable stress within individuals, not through rapid societal change, but by intentional physical debilitation and continual accusations, cross-examinations, and use of other anxiety-provoking techniques. Then they offer their prisoners an escape from the induced psychological torment: conversion to the new gospel.[12]

The similarity in the mental processes involved in "brainwashing" and in the formation of nativistic movements becomes even clearer upon examination of the Chinese Communist attempt to establish their doctrines in mainland China. Again, the Communists intentionally have created conditions like those out of which nativistic cults have arisen more spontaneously in other societies. In addition to the stress which ordinarily would accompany rapid industrialization of an economically backward society, the Chinese leaders have provoked additional anxiety through the systematic use of group confessions and denunciations and have intentionally disrupted family life. Hostility toward the American enemy has been purposely aroused and used to unify the masses, as well as to justify the repression of millions of alleged internal enemies. The whole population has been continually urged to repent their sins and to adopt wholeheartedly the Com-

11 Linton, 237. Also, Carroll L. Riley and John Hobgood, "A Recent Nativistic Movement Among the Southern Tepehuan Indians," *Southwest Journal of Anthropology*, XV (1959), 355–60.

12 Robert J. Lifton, "Thought Reform in Western Civilians in Chinese Communist Prisons," *Psychiatry*, XIX (1956), 173–95; Edgar H. Schein, "The Chinese Indoctrination Program for Prisoners of War, A Study of Attempted Brainwashing," *Psychiatry*, XIX (1956), 149–72.

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munist gospel, which has a strong nativistic component. As a psychologist has remarked, to a large extent the Chinese Communists provide both the disease and the cure.[13]

The ferocious outbreak of nativism in the United States after World War I was not consciously planned or provoked by any individual or group, although some Americans took advantage of the movement once it started. Rather, the Red Scare, like the Gaiwiio and Boxer movements described above, was brought on largely by a number of severe social and economic dislocations which threatened the national equilibrium. The full extent and the shocking effects of these disturbances of 1919 have not yet been adequately described. Runaway prices, a brief but sharp stock market crash and business depression, revolutions throughout Europe, widespread fear of domestic revolt, bomb explosions, and an outpouring of radical literature were distressing enough. These sudden difficulties, moreover, served to exaggerate the disruptive effects already produced by the social and intellectual ravages of the World War and the preceding reform era, and by the arrival, before the war, of millions of new immigrants. This added stress intensified the hostility of Americans strongly antagonistic to minority groups, and brought new converts to blatant nativism from among those who ordinarily were not overtly hostile toward radicals or recent immigrants.

Citizens who joined the crusade for one hundred per cent Americanism sought, primarily, a unifying force which would halt the apparent disintegration of their culture. The movement, they felt, would eliminate those foreign influences which the one hundred per centers believed were the major cause of their anxiety.

Many of the postwar sources of stress were also present during World War I, and the Red Scare, as John Higham has observed, was partly an exaggeration of wartime passions.[14] In 1917–18 German-Americans served as the object of almost all our nativistic fervor; they were the threatening

13 Edgar H. Schein, with Inge Schneier and Curtis H. Bark, *Coercive Persuasion* (New York, 1961); William Sargent, *Battle for the Mind* (New York, 1957), 150–65; Robert J. Lifton, *Thought Reform and the Psychology of Totalism* (New York, 1961); R. L. Walker, *China Under Communism* (London, 1946).

14 Higham, 222.

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intruders who refused to become good citizens. "They used America," a patriotic author declared in 1918 of two million German-Americans, "they never loved her. They clung to their old language, their old customs, and cared nothing for ours. . . . As a class they were clannish beyond all other races coming here." [15] Fear of subversion by German agents was almost as extravagant in 1917–18 as anxiety about "reds" in the postwar period. Attorney General Thomas Watt Gregory reported to a friend in May 1918 that "we not infrequently receive as many as fifteen hundred letters in a single day suggesting disloyalty and the making of investigations." [16]

Opposition to the war by radical groups helped smooth the transition among American nativists from hatred of everything German to fear of radical revolution. The two groups of enemies were associated also for other reasons. High government officials declared after the war that German leaders planned and subsidized the Bolshevik Revolution.[17] When bombs blasted homes and public buildings in nine cities in June 1919, the director of the Justice Department's Bureau of Investigation asserted that the bombers were "connected with Russian bolshevism, aided by Hun money." [18] In November 1919, a year after the armistice, a popular magazine warned of "the Russo-German movement that is now trying to dominate America. . . ." [19]

Even the wartime hostility toward German-Americans, however, is more understandable when seen in the light of

15 Emerson Hough, *The Web* (Chicago, 1919), 23. Hough was a rabid one hundred per center during the Red Scare also.

16 T. W. Gregory to R. E. Vinson, May 13, 1918, Papers of Thomas Watt Gregory (Library of Congress, Washington, D. C.).

17 Subcommittee of Senate Committee on the Judiciary, *Hearings, Brewing and Liquor Interests and German and Bolshevik Propaganda*, 66th Congress, 1st Session, 1919, 2669 ff.; *The New York Times*, July 7, August 11 and 29, September 15–21, 1918.

18 *Washington Post*, July 3, 1919. Bureau Director William J. Flynn produced no evidence to back this assertion. Later he claimed to have conclusive proof that the bombers were Italian anarchists. Flynn to Attorney General Harry Daugherty, April 4, 1922, Department of Justice Records, File 202600, Sect. 5 (National Archives, Washington, D. C.).

19 *Saturday Evening Post*, CXCII (November 1, 1919), 28. For similar assertions in other publications, Meno Lovenstein, *American Opinion of Soviet Russia* (Washington, D. C., 1941), Chap. 1, *passim*.

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recent anthropological and psychological studies. World War I disturbed Americans not only because of the real threat posed by enemy armies and a foreign ideology. For many citizens it had the further effect of shattering an already weakened intellectual tradition. When the European governments decided to fight, they provided shocking evidence that man was not, as most educated members of Western society had believed, a rational creature progressing steadily, if slowly, toward control of his environment. When the great powers declared war in 1914, many Americans as well as many Europeans were stunned. The *New York Times* proclaimed a common theme—European civilization had collapsed: The supposedly advanced nations, declared the *Times,* "have reverted to the condition of savage tribes roaming the forests and falling upon each other in a fury of blood and carnage to achieve the ambitious designs of chieftains clad in skins and drunk with mead." [20] Franz Alexander, director for twenty-five years of the Chicago Institute of Psychoanalysis, recently recalled his response to the outbreak of the World War:

> The first impact of this news is [*sic*] unforgettable. It was the sudden intuitive realization that a chapter of history had ended. . . . Since then, I have discussed this matter with some of my contemporaries and heard about it a great deal in my early postwar psychoanalytic treatments of patients. To my amazement, the others who went through the same events had quite a similar reaction. . . . It was an immediate vivid and prophetic realization that something irrevocable of immense importance had happened in history.[21]

Americans were jolted by new blows to their equilibrium after entering the war. Four million men were drafted away from familiar surroundings and some of them experienced

[20] Quoted in William E. Leuchtenburg, *The Perils of Prosperity, 1914–32* (Chicago, 1958), 13. There is no comprehensive study of the effects of the war on the American mind. For brief treatments, Henry F. May, *The End of American Innocence* (New York, 1959), 361–67; Merle Curti, *The Growth of American Thought* (New York, 1951), 687–705; Ralph Henry Gabriel, *The Course of American Democratic Thought* (New York, 1956), 387, 404; André Siegfried, *America Comes of Age* (New York, 1927), 3; Walter Lord, *The Good Years, From 1900 to the First World War* (New York, 1960), 339–41.

[21] Franz Alexander, *The Western Mind in Transition* (New York, 1960), 73–74. Also see William Barrett, *Irrational Man* (Garden City, N. Y., 1961), 32–33.

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the terrible carnage of trench warfare. Great numbers of women left home to work in war industries or to replace men in other jobs. Negroes flocked to Northern industrial areas by the hundreds of thousands, and their first mass migration from the South created violent racial antagonism in Northern cities.

During the war, also, Americans sanctioned a degree of government control over the economy which deviated sharply from traditional economic individualism. Again, fears aroused before the war were aggravated, for the reform legislation of the Progressive era had tended to increase government intervention, and many citizens were further perturbed by demands that the federal government enforce even higher standards of economic and social morality. By 1919, therefore, some prewar progressives as well as conservatives feared the gradual disappearance of highly valued individual opportunity and responsibility. Their fears were fed by strong postwar calls for continued large-scale government controls—extension of federal operation of railroads and of the Food Administration, for example.

The prime threat to these long-held individualistic values, however, and the most powerful immediate stimulus to the revitalistic response, came from Russia. There the Bolshevik conquerors proclaimed their intention of exporting Marxist ideology. If millions of Americans were disturbed in 1919 by the specter of communism, the underlying reason was not fear of foreign invasion—Russia, after all, was still a backward nation recently badly defeated by German armies. The real threat was the potential spread of communist ideas. These, the one hundred per centers realized with horror, possessed a genuine appeal for reformers and for the economically underprivileged, and if accepted they would complete the transformation of America.

A clear picture of the Bolshevik tyranny was not yet available; therefore, as after the French Revolution, those who feared the newly successful ideology turned to fight the revolutionary ideals. So the *Saturday Evening Post* declared editorially in November 1919 that "History will see our present state of mind as one with that preceding the burning of witches, the children's crusade, the great tulip craze and other examples of softening of the world brain." The *Post*

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referred not to the Red Scare or the impending Palmer Raids, but to the spread of communist ideology. Its editorial concluded: "The need of the country is not more idealism, but more pragmatism; not communism, but common sense." [22] One of the most powerful patriotic groups, the National Security League, called upon members early in 1919 to "teach 'Americanism.' This means the fighting of Bolshevism . . . by the creation of well defined National Ideals." Members "must preach Americanism and instil the idealism of America's Wars, and that American spirit of service which believes in giving as well as getting." [23] New York attorney, author, and educator Henry Waters Taft warned a Carnegie Hall audience late in 1919 that Americans must battle "a propaganda which is tending to undermine our most cherished social and political institutions and is having the effect of producing widespread unrest among the poor and the ignorant, especially those of foreign birth." [24]

When the war ended Americans also confronted the disturbing possibility, pointed up in 1919 by the struggle over the League of Nations, that Europe's struggles would continue to be their own. These factors combined to make the First World War a traumatic experience for millions of citizens. As Senator James Reed of Missouri observed in August 1919, "This country is still suffering from shell shock. Hardly anyone is in a normal state of mind. . . . A great storm has swept over the intellectual world and its ravages and disturbances still exist." [25]

The wartime "shell shock" left many Americans extraordinarily susceptible to psychological stress caused by postwar social and economic turbulence. Most important for the course of the Red Scare, many of these disturbances had their greatest effect on individuals already antagonistic toward minorities. First of all, there was some real evidence of danger to the nation in 1919, and the nation provided the chief

22 *Saturday Evening Post,* CXCII (November 1, 1919), 28.

23 National Security League, *Future Work* (New York, 1919), 6.

24 Henry Waters Taft, *Aspects of Bolshevism and Americanism, Address before the League for Political Education at Carnegie Hall, New York, December 6, 1919* (New York, 1919), 21.

25 U. S., *Congressional Record,* 66th Congress, 1st Session, August 15, 1919, 3892.

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emotional support for many Americans who responded easily to charges of an alien radical menace. Violence flared throughout Europe after the war and revolt lifted radicals to power in several Eastern and Central European nations. Combined with the earlier Bolshevik triumph in Russia these revolutions made Americans look more anxiously at radicals here. Domestic radicals encouraged these fears; they became unduly optimistic about their own chances of success and boasted openly of their coming triumph. Scores of new foreign language anarchist and communist journals, most of them written by and for Southern and Eastern European immigrants, commenced publication, and the established radical press became more exuberant. These periodicals never tired of assuring readers in 1919 that "the United States seems to be on the verge of a revolutionary crisis." [26] American newspapers and magazines reprinted selections from radical speeches, pamphlets, and periodicals so their readers could see what dangerous ideas were abroad in the land.[27] Several mysterious bomb explosions and bombing attempts, reported in bold front page headlines in newspapers across the country, frightened the public in 1919. To many citizens these seemed part of an organized campaign of terror carried on by alien radicals intending to bring down the federal government. The great strikes of 1919 and early 1920 aroused similar fears.[28]

Actually American radical organizations in 1919 were dis-

[26] Robert E. Park, *The Immigrant Press and Its Control* (New York, 1922), 214, 230–38, 241–45; R. E. Park and Herbert A. Miller, *Old World Traits Transplanted* (New York, 1921), 99–101; Daniel Bell, "The Background and Development of Marxian Socialism in the United States," in Donald Drew Egbert and Stow Persons, *Socialism in American Life* (Princeton, 1952), I, 334; Lovenstein, 7–50; Leuchtenburg, 67–68; Murray, 33–36.

[27] The Justice Department distributed pamphlets containing such material to all American newspapers and magazines; *Red Radicalism, as Described by Its Own Leaders* (Washington, D. C., 1920); National Popular Government League, *To the American People, Report Upon the Illegal Practices of the Department of Justice* (Washington, D. C., 1920), 64–66. The staunchly anti-radical *New York Times* published translations from a large sample of foreign language radical newspapers on June 8, 1919.

[28] Murray, Chaps. 5, 7–10. Asked by a congressional committee a few weeks after the spate of bombings in June 1919 whether there was real evidence of an organized effort to destroy the federal government, Assistant Attorney General Francis P. Garvan replied, "Certainly." Garvan was in charge of federal prosecution of radicals. *Washington Post*, June 27, 1919.

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organized and poverty-stricken. The Communists were inept, almost without contact with American workers and not yet dominated or subsidized by Moscow. The IWW was shorn of its effective leaders, distrusted by labor, and generally declining in influence and power. Violent anarchists were isolated in a handful of tiny, unconnected local organizations.[29] One or two of these anarchist groups probably carried out the "bomb conspiracy" of 1919; but the extent of the "conspiracy" can be judged from the fact that the bombs killed a total of two men during the year, a night watchman and one of the bomb throwers, and seriously wounded one person, a maid in the home of a Georgia senator.[30]

Nevertheless, prophesies of national disaster abounded in 1919, even among high government officials. Secretary of State Robert Lansing confided to his diary that we were in real peril of social revolution. Attorney General A. Mitchell Palmer advised the House Appropriations Committee that "on a certain day, which we have been advised of," radicals would attempt "to rise up and destroy the Government at one fell swoop." Senator Charles Thomas of Colorado warned that "the country is on the verge of a volcanic upheaval." And Senator Miles Poindexter of Washington declared, "There is real danger that the government will fall." [31] A West Virginia wholesaler, with offices throughout the state, informed the Justice Department in October 1919 that "there is hardly a respectable citizen of my acquaintance who does not believe that we are on the verge of armed conflict in this country." William G. McAdoo was told by a

29 Theodore Draper, *The Roots of American Communism* (New York, 1957), 198–200, 302, 312–14; David J. Saposs, *Left Wing Unionism, A Study in Policies and Tactics* (New York, 1926), 49–50, 152–57; Selig Perlman and Philip Taft (eds.), *Labor Movements* in John R. Commons (ed.), *History of Labour in the United States 1896–1932*, IV (New York, 1935), 621, 431–32; Jerome Davis, *The Russian Immigrant* (New York, 1922), 114–18; Kate Holladay Claghorn, *The Immigrant's Day in Court* (New York, 1923), 363–73; John S. Gambs, *The Decline of the I.W.W.* (New York, 1932), 133; Murray, 107–10.

30 *The New York Times*, May 1, June 3, 4, 1919.

31 "The Spread of Bolshevism in the United States," private memorandum, dated July 26, 1919, Papers of Robert Lansing (Library of Congress, Washington, D. C.); "One Point of View of the Murders at Centralia, Washington," private memorandum, dated November 13, 1919, Lansing Papers; U. S., *Congressional Record*, 66th Congress, 1st Session, October 14, 1919, 6869; *Washington Post*, February 16, 1919; New York *World*, June 19, 1919.

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trusted friend that "Chicago, which has always been a very liberal minded place, seems to me to have gone mad on the question of the 'Reds.'" Delegates to the Farmers National Congress in November 1919 pledged that farmers would assist the government in meeting the threat of revolution.[32]

The slight evidence of danger from radical organizations aroused such wild fear only because Americans had already encountered other threats to cultural stability. However, the dislocations caused by the war and the menace of communism alone would not have produced such a vehement nativistic response. Other postwar challenges to the social and economic order made the crucial difference.

Of considerable importance was the skyrocketing cost of living. Retail prices more than doubled between 1915 and 1920, and the price rise began gathering momentum in the spring of 1919.[33] During the summer of 1919 the dominant political issue in America was not the League of Nations; not even the "red menace" or the threat of a series of major strikes disturbed the public as much as did the climbing cost of living. The *Washington Post* early in August 1919 called rising prices, "the burning domestic issue. . . ." Democratic National Chairman Homer Cummings, after a trip around the country, told President Woodrow Wilson that more Americans were worried about prices than about any other public issue and that they demanded government action. When Wilson decided to address Congress on the question the Philadelphia *Public Ledger* observed that the administration had "come rather tardily to a realization of what is uppermost in the minds of the American people." [34]

Then the wave of postwar strikes—there were 3,600 of

[32] Henry Barham to Palmer, October 27, 1919, Justice Department Records, File 202600; unidentified correspondent to McAdoo, February 10, 1920, McAdoo Papers (Library of Congress, Washington, D. C.); A. P. Sanders to Palmer, November 12, 1919, Justice Department Records, File 202600; *The New York Times,* October 31, 1919.

[33] U. S. Bureau of the Census, *Historical Statistics of the United States, Colonial Times to 1952, A Statistical Abstract Supplement* (Washington, D. C., 1960), 91, 92, 126; U. S. Department of Labor, Bureau of Labor Statistics, Bulletin Number 300, *Retail Prices 1913 to December, 1920* (Washington, D. C., 1922), 4; Daniel J. Ahearn, Jr., *The Wages of Farm and Factory Laborers 1914–1944* (New York, 1945), 227.

[34] *Washington Post,* August 1, 4, 1919; *The New York Times,* July 30, August 1, 1919; Philadelphia *Public Ledger,* August 5, 1919.

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them in 1919 involving over 4,000,000 workers [35]—reached a climax in the fall of 1919. A national steel strike began in September and nationwide coal and rail walkouts were scheduled for November 1. Unions gained in membership and power during the war, and in 1919 labor leaders were under strong pressure to help workers catch up to or go ahead of mounting living costs. Nevertheless, influential government officials attributed the walkouts to radical activities. Early in 1919, Secretary of Labor William B. Wilson declared in a public speech that recent major strikes in Seattle, Butte, Montana, and Lawrence, Massachusetts, had been instituted by the Bolsheviks and the IWW for the sole purpose of bringing about a nationwide revolution in the United States.[36] During the steel strike of early fall, 1919, a Senate investigating committee reported that "behind this strike there is massed a considerable element of I.W.W.'s, anarchists, revolutionists, and Russian soviets. . . ." [37] In April 1920 the head of the Justice Department's General Intelligence Division, J. Edgar Hoover, declared in a public hearing that at least fifty per cent of the influence behind the recent series of strikes was traceable directly to communist agents.[38]

Furthermore, the nation suffered a sharp economic depression in late 1918 and early 1919, caused largely by sudden cancellations of war orders. Returning servicemen found it difficult to obtain jobs during this period, which coincided with the beginning of the Red Scare. The former soldiers had been uprooted from their homes and told that they were engaged in a patriotic crusade. Now they came back to find "reds" criticizing their country and threatening the government with violence, Negroes holding good jobs in the big cities, prices terribly high, and workers who had not served in

35 Florence Peterson, *Strikes in the United States, 1880–1936*, U. S. Department of Labor Bulletin Number 651 (Washington, D. C., 1938), 21. More employees engaged in strikes in 1919 than the total over the ten-year period 1923–32.

36 *Washington Post*, February 21, 1919. As late as April 1920, Secretary Wilson agreed with Palmer during a Cabinet meeting that the nationwide rail walkout had been caused by Communists and the IWW. Entry in Josephus Daniels' Diary for April 14, 1920, Papers of Josephus Daniels (Library of Congress, Washington, D. C.).

37 U. S. Senate, Committee on Education and Labor, *Report, Investigation of Strike in Steel Industry*, 66th Congress, 1st Session, 1919, 14.

38 *The New York Times*, April 25, 1920, 23.

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the armed forces striking for higher wages.[39] A delegate won prolonged applause from the 1919 American Legion Convention when he denounced radical aliens, exclaiming, "Now that the war is over and they are in lucrative positions while our boys haven't a job, we've got to send those scamps to hell." The major part of the mobs which invaded meeting halls of immigrant organizations and broke up radical parades, especially during the first half of 1919, was comprised of men in uniform.[40]

A variety of other circumstances combined to add even more force to the postwar nativistic movement. Long before the new immigrants were seen as potential revolutionists they became the objects of widespread hostility. The peak of immigration from Southern and Eastern Europe occurred in the fifteen years before the war; during that period almost ten million immigrants from those areas entered the country. Before the anxious eyes of members of all classes of Americans, the newcomers crowded the cities and began to disturb the economic and social order.[41] Even without other postwar disturbances a nativistic movement of some strength could have been predicted when the wartime solidarity against the German enemy began to wear off in 1919.

In addition, not only were the European revolutions most successful in Eastern and to a lesser extent in Southern Europe, but aliens from these areas predominated in American radical organizations. At least ninety per cent of the members of the two American Communist parties formed in 1919 were born in Eastern Europe. The anarchist groups whose literature and bombs captured the imagination of the American public in 1919 were composed almost entirely of Italian, Spanish, and Slavic aliens. Justice Department announce-

39 George Soule, *Prosperity Decade, From War to Depression: 1917–1929* (New York, 1947), 81–84; Murray, 125, 182–83.

40 *Proceedings and Committees, Caucus of the American Legion* (St. Louis, 1919), 117; *The New York Times,* May 2, 1919; *Washington Post,* May 2, 1919. Ex-servicemen also played major roles in the great Negro-white race riots of mid-1919. *Washington Post,* July 20–23, 28–31.

41 *Historical Statistics of the United States,* 56. On the causes of American hostility to recent immigrants see John Higham's probing and provocative essay "Another Look at Nativism," *Catholic Historical Review,* XLIV (1958), 147–58. Higham stresses status conflicts, but does not explain why some competitors on the crowded social ladder were much more antagonistic to the new immigrants than were others.

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ments and statements by politicians and the press stressed the predominance of recent immigrants in radical organizations.[42] Smoldering prejudice against new immigrants and identification of these immigrants with European as well as American radical movements, combined with other sources of postwar stress to create one of the most frenzied and one of the most widespread nativistic movements in the nation's history.

The result, akin to the movements incited by the Chinese Boxers or the Indian Ghost Dancers, was called Americanism or one hundred per cent Americanism.[43] Its objective was to end the apparent erosion of American values and the disintegration of American culture. By reaffirming those beliefs, customs, symbols, and traditions felt to be the foundation of our way of life, by enforcing conformity among the population, and by purging the nation of dangerous foreigners, the one hundred per centers expected to heal societal divisions and to tighten defenses against cultural change.

Panegyrics celebrating our history and institutions were delivered regularly in almost every American school, church, and public hall in 1919 and 1920. Many of these fervent addresses went far beyond the usual patriotic declarations. Audiences were usually urged to join a crusade to protect our hallowed institutions. Typical of the more moderate statements was Columbia University President Nicholas Murray Butler's insistence in April 1919 that "America will be saved, not by those who have only contempt and despite for her founders and her history, but by those who look with respect and reverence upon the great series of happenings extending from the voyage of the Mayflower. . . ." [44]

What one historian has called "a riot of biographies of

[42] Draper, 189–90; *Annual Report of the Attorney General for 1920* (Washington, D.C., 1920), 177; Higham, *Strangers in the Land*, 226–27.

[43] The word "Americanism" was used by the nativists of the eighteen-forties and eighteen-fifties. During World War I, the stronger phrase "100 per cent Americanism" was invented to suit the belligerent drive for universal conformity.

[44] Horace M. Kallen, *Culture and Democracy in the United States* (New York, 1924), Chap. 3, 154–55; Edward G. Hartman, *The Movement to Americanize the Immigrant* (New York, 1948), Chap. 9; Nicholas Murray Butler, *Is America Worth Saving? An Address Delivered Before the Commercial Club of Cincinnati, Ohio, April 19, 1919* (New York, 1919), 20.

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American heroes—statesmen, cowboys, and pioneers" [45] appeared in this brief period. Immigrants as well as citizens produced many autobiographical testimonials to the superiority of American institutions. These patriotic tendencies in our literature were as short-lived as the Red Scare, and have been concealed by "debunking" biographies of folk heroes and skeptical autobiographies so common later in the nineteen-twenties. An unusual number of motion pictures about our early history were turned out immediately after the war and the reconstruction of colonial Williamsburg and of Longfellow's Wayside Inn was begun. With great fanfare, Secretary of State Lansing placed the original documents of the Constitution and the Declaration of Independence on display in January 1920, and the State Department distributed movies of this ceremony to almost every town and city in the United States.[46] Organizations like the National Security League, the Association for Constitutional Government, the Sons and the Daughters of the American Revolution, the Colonial Dames of America, with the cooperation of the American Bar Association and many state Bar Associations, organized Constitution Day celebrations and distributed huge numbers of pamphlets on the subject throughout the country.

The American flag became a sacred symbol. Legionaires demanded that citizens "Run the Reds out from the land whose flag they sully." [47] Men suspected of radical leanings were forced to kiss the stars and stripes. A Brooklyn truck driver decided in June 1919 that it was unpatriotic to obey a New York City law obliging him to fly a red cloth on lumber which projected from his vehicle. Instead he used as a danger signal a small American flag. A policeman, infuriated at the sight of the stars and stripes flying from a lumber pile, arrested the driver on a charge of disorderly conduct. Despite the Brooklyn patriot's insistence that he meant no of-

[45] Emerson Hunsberger Loucks, *The Ku Klux Klan in Pennsylvania* (New York, 1936), 163.

[46] Kallen, Chap. 3, 154–55; Division of Foreign Intelligence, "Memorandum about Constitution Ceremonies," January 19, 1920, Lansing Papers; *The New York Times*, January 18, 1920.

[47] *American Legion Weekly*, I (November 14, 1919), 12.

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fense to the flag, he was reprimanded and fined by the court.[48]

Recent immigrants, especially, were called upon to show evidence of real conversion. Great pressure was brought to bear upon the foreign-born to learn English and to forget their native tongues. As Senator William S. Kenyon of Iowa declared in October 1919, "The time has come to make this a one-language nation." [49] An editorial in the *American Legion Weekly* took a further step and insisted that the one language must be called "American. Why even in Mexico they do not stand for calling the language the Spanish language." [50]

Immigrants were also expected to adopt our customs and to snuff out remnants of Old World cultures. Genteel prewar and wartime movements to speed up assimilation took on a "frightened and feverish aspect." [51] Welcoming members of an Americanization conference called by his department, Secretary of the Interior Franklin K. Lane exclaimed in May 1919, "You have been gathered together as crusaders in a great cause. . . . There is no other question of such importance before the American people as the solidifying and strengthening of true American sentiment." A Harvard University official told the conference that "The Americanization movement . . . gives men a new and holy religion. . . . It challenges each one of us to a renewed consecration and devotion to the welfare of the nation." [52] The National Security League boasted, in 1919, of establishing one thousand study groups to teach teachers how to inculcate "Americanism" in their foreign-born students.[53] A critic of the prevailing mood protested against "one of our best advertised American mottoes, 'One country, one language, one flag,'" which, he complained, had become the basis for a fervent nationwide program.[54]

48 Sullivan, VI, 118; New York *World*, June 22, 1919.

49 *The New York Times*, October 14, 1919.

50 *American Legion Weekly*, I (November 14, 1919), 12.

51 Higham, *Strangers in the Land*, 225.

52 United States Department of the Interior, Bureau of Education, *Organization Conference, Proceedings* (Washington, D. C., 1919), 293, 345–50.

53 National Security League, 4.

54 *Addresses and Proceedings of the Knights of Columbus Educational Con-*

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As the postwar movement for one hundred per cent Americanism gathered momentum, the deportation of alien nonconformists became increasingly its most compelling objective. Asked to suggest a remedy for the nationwide upsurge in radical activity, the Mayor of Gary, Indiana, replied, "Deportation is the answer, deportation of these leaders who talk treason in America and deportation of those who agree with them and work with them." "We must remake America," a popular author averred, "We must purify the source of America's population and keep it pure. . . . We must insist that there shall be an American loyalty, brooking no amendment or qualification." [55] As Higham noted, "In 1919, the clamor of 100 per centers for applying deportation as a purgative arose to an hysterical howl. . . . Through repression and deportation on the one hand and speedy total assimilation on the other, 100 per centers hoped to eradicate discontent and purify the nation." [56]

Politicians quickly sensed the possibilities of the popular frenzy for Americanism. Mayor Ole Hanson of Seattle, Governor Calvin Coolidge of Massachusetts, and General Leonard Wood became the early heroes of the movement.[57] The man in the best political position to take advantage of the popular feeling, however, was Attorney General A. Mitchell Palmer.[58] In 1919, especially after the President's physical collapse, only Palmer had the authority, staff, and money necessary to arrest and deport huge numbers of radical aliens. The most virulent phase of the movement for one hundred

vention (New Haven, 1919), 71. Again note the family resemblance between the attempt to protect America through absolute conformity in 1919–20 and the more drastic, centrally-planned Chinese Communist efforts at national indoctrination. A student of Chinese "coercive persuasion" described the "elaborate unanimity rituals like parades, . . . 'spontaneous' mass demonstrations and society-wide campaigns, the extensive proselytizing among the 'heretics' or the 'infidels,' the purges, programs of re-education, and other repressive measures aimed at deviants." In China, also, past national glory is invoked as evidence of present and future greatness. Schein *et al.*, 62; Lifton, *Thought Reform and the Psychology of Totalism;* Walker, *China Under Communism.*

55 Emerson Hough, "Round Our Town," *Saturday Evening Post,* CXCII (February 21, 1920), 102; Hough, *The Web,* 456.

56 Higham, *Strangers in the Land,* 227, 255.

57 Murray, 62–65, 147–48, 159–60.

58 For a full discussion of Palmer's role, Stanley Coben, *A. Mitchell Palmer: Politician* (New York, 1963).

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per cent Americanism came early in 1920, when Palmer's agents rounded up for deportation over six thousand aliens and prepared to arrest thousands more suspected of membership in radical organizations. Most of these aliens were taken without warrants, many were detained for unjustifiably long periods of time, and some suffered incredible hardships. Almost all, however, were eventually released.[59]

After Palmer decided that he could ride the postwar fears into the presidency, he set out calculatingly to become the symbol of one hundred per cent Americanism. The Palmer raids, his anti-labor activities, and his frequent pious professions of patriotism during the campaign were all part of this effort. Palmer was introduced by a political associate to the Democratic party's annual Jackson Day dinner in January 1920 as "an American whose Americanism cannot be misunderstood." In a speech delivered in Georgia shortly before the primary election (in which Palmer won control of the state's delegation to the Democratic National Convention), the Attorney General asserted: "I am myself an American and I love to preach my doctrine before undiluted one hundred per cent Americans, because my platform is, in a word, undiluted Americanism and undying loyalty to the republic." The same theme dominated the address made by Palmer's old friend, John H. Bigelow of Hazleton, Pennsylvania, when he placed Palmer's name in nomination at the 1920 National Convention. Proclaimed Bigelow: "No party could survive today that did not write into its platform the magic word 'Americanism.' . . . The Attorney-General of the United States has not merely professed, but he has proved his true Americanism. . . . Behind him I see a solid phalanx of true Americanism that knows no divided allegiance." [60]

Unfortunately for political candidates like Palmer and

[59] Coben, *Palmer,* Chaps. 11, 12; Claghorn, Chap. 10; Constantine Panunzio, *The Deportation Cases of 1919–1920* (New York, 1920); Zechariah Chafee, Jr., *Free Speech in the United States* (Cambridge, 1941), 204–17; Murray, Chap. 13.

[60] Coben, *Palmer,* Chap. 13; *The New York Times,* January 9, 1920; Atlanta *Constitution,* April 7, 1920; *Official Report of the Proceedings of the Democratic National Convention, 1920* (Indianapolis, 1920), 113–14. Palmer also launched a highly publicized campaign to hold down soaring prices in 1919–20, by fixing retail prices and bringing suits against profiteers and hoarders.

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Wood, most of the social and economic disturbances which had activated the movement they sought to lead gradually disappeared during the first half of 1920. The European revolutions were put down; by 1920 communism seemed to have been isolated in Russia. Bombings ceased abruptly after June 1919, and fear of new outrages gradually abated. Prices of food and clothing began to recede during the spring. Labor strife almost vanished from our major industries after a brief railroad walkout in April. Prosperity returned after mid-1919 and by early 1920 business activity and employment levels exceeded their wartime peaks.[61] At the same time, it became clear that the Senate would not pass Wilson's peace treaty and that America was free to turn its back on the responsibilities of world leadership. The problems associated with the new immigrants remained; so did the disillusionment with Europe and with many old intellectual ideals. Nativism did not disappear from the American scene; but the frenzied attempt to revitalize the culture did peter out in 1920. The handful of unintimidated men, especially Assistant Secretary of Labor Louis F. Post, who had used the safeguards provided by American law to protect many victims of the Red Scare, found increasing public support. On the other hand, politicians like Palmer, Wood, and Hanson were left high and dry, proclaiming the need for one hundred per cent Americanism to an audience which no longer urgently cared.

It is ironic that in 1920 the Russian leaders of the Comintern finally took charge of the American Communist movement, provided funds and leadership, and ordered the Communist factions to unite and participate actively in labor organizations and strikes. These facts were reported in the American press.[62] Thus a potentially serious foreign threat to national security appeared just as the Red Scare evaporated, providing a final illustration of the fact that the frenzied one hundred per centers of 1919–20 were affected less by the "red menace" than by a series of social and economic dislocations.

Although the Red Scare died out in 1920, its effects lin-

[61] Bell, 334; Soule, 83–88; *Seventh Annual Report of the Federal Reserve Board for the Year 1920* (Washington, D. C., 1920), 7.

[62] Draper, 244, 267–68; New York *World*, March 29, 1920.

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gered. Hostility toward immigrants, mobilized in 1919–20, remained strong enough to force congressional passage of restrictive immigration laws. Some of the die-hard one hundred per centers found a temporary home in the Ku Klux Klan until that organization withered away during the mid-twenties. As its most lasting accomplishments, the movement for one hundred per cent Americanism fostered a spirit of conformity in the country, a satisfaction with the *status quo,* and the equation of reform ideologies with foreign enemies. Revitalization movements have helped many societies adapt successfully to new conditions. The movement associated with the American Red Scare, however, had no such effect. True, it unified the culture against the threats faced in 1919–20; but the basic problems—a damaged value system, an unrestrained business cycle, a hostile Russia, and communism—were left for future generations of Americans to deal with in their own fashion.

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POLICY BY DEFAULT: THE ORIGIN AND FATE OF THE PRESCOTT LETTER

IN RECENT years certain misconceptions have crept into discussions of American policy toward polar regions in this century. It is said, for example, that before the negotiation of the Antarctic Treaty of 1959 American policy toward Antarctica was no more or less than a policy of abstaining from making any claims to territory in Antarctica, while at the same time denying the validity of any claims asserted by other countries to territory in this region. This, it is said, is the significance of the policy enunciated in the now famous Prescott letter. In one passage of this letter which secretary of State Charles Evans Hughes sent to A. W. Prescott on May 13, 1924, the Secretary of State wrote: "It is the opinion of the Department that the discovery of lands unknown to civilization, even when coupled with a formal taking of possession, does not support a valid claim of sovereignty unless the discovery is followed by an actual settlement of the discovered country." [1] Since the overwhelming portion of polar lands was regarded as uninhabitable, this passage was taken to mean that the United States denied the possibility of *any* nation's possessing any portion of polar lands.

Reading the Prescott letter this way—taking it to mean what it literally says—American policy is criticized for being negative, legalistic, and lacking in vision.[2] It is negative in that it appears to put the United States in the position of asserting that no change in the status of polar lands is pos-

[1] The Secretary of State to A. W. Prescott, May 13, 1924, DoS File 811.014/101. The file number reference is to the records of the Department of State on file in the Foreign Affairs Branch of the National Archives.

[2] These criticisms were all given a full airing by witnesses appearing before the Senate Committee on Foreign Relations at the time of the hearings on the Antarctic Treaty. See U. S. Senate, Committee on Foreign Relations, *Hearings, Antarctic Treaty*, 86th Congress, 2nd Session, 1960, especially pp. 3–21.

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sible. It is negative also in that the United States, while denying the possibility of any nation's possessing land in the polar regions because the peculiar climatic conditions make effective occupation impossible, abstains from making any suggestion of a solution to this difficulty so that nations might enjoy the potential benefits of the polar regions. It is criticized also for being legalistic. The doctrine of the Prescott letter reads like a statement from a contemporary treatise on international law, which indeed it could have been. It seemed as if the United States was adhering to a rule of international law (the requirement of effective occupation) that was obviously inappropriate to the polar regions, thus pushing the ideal of law-abidingness to absurd lengths. Finally, our policy is said to be lacking in vision because those who were responsible for it either failed to consider the economic and military potentialities of Antarctica, or failed to design a status—along the lines of the Antarctic Treaty of 1959, for example—appropriate to an area with the peculiar qualities which the Prescott letter emphasized.

There is a good deal of justice behind these criticisms, for American policy of the nineteen-twenties and after does often seem to bear the characteristics complained of. But the criticism falls somewhat wide of the mark in that it rests on two assumptions which a close examination of the evolution of policy toward the Antarctic in the twenties reveals to be false: (1) that American policy of the nineteen-twenties and after was planned that way, that it was the result of sticking closely to a line of policy the dimensions of which were settled by April 1924, the time of the writing of the Prescott letter, and (2) that the Prescott letter was the vehicle chosen to make this line of policy public. Both assumptions are false; to hold them is to mistake the circumstances under which the Prescott letter was writen and, even more, to mistake the purposes for which that letter was designed. It is to set forth these circumstances and to clarify those purposes this study has been undertaken.

II

Up to the end of the First World War, the record of American interest and activity in the polar regions was a curious one. On the one hand, the record showed long-continuing

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participation by its citizens in the development of both the south and north polar regions. So many American explorers, scientists, and fishermen actively at work in the polar regions seem to evidence a real American interest in the polar regions. On the other hand, in seeming refutation of the interests of its citizens, the American government made no moves to lay claim to any lands in the polar regions nor did it protest British assertions of sovereignty over polar areas or similar claims to the Arctic Islands first made by the Russian government in 1916.

This, then, was roughly the situation that existed at the end of the First World War; it was against this background that policy-makers in the Department of State found themselves trying to define the interest of the United States with respect to the acquisition of polar territory, as each new question presented itself during the nineteen-twenties.

In the first few years after the end of World War I the Department of State was left relatively free to ignore matters concerning polar regions, as it had under the press of wartime business. Not until 1922 did any matter arise which required the department to attempt to locate its interest in the development of polar lands. Then, as was almost invariably the case with polar matters in this period, the department moved into action, not because of initiative from within, but because of pressure from without.

III

In February 1922 Mr. D. C. Poole, acting chief of the Division of Russian Affairs, received a letter dated February 6, 1922,[3] from John B. Burnham, president of the American Game Protective and Propagation Association, dealing with the subject of Wrangel Island.[4] Mr. Burnham was not unknown to the department. Since the previous summer, when an expedition consisting of four men and one Eskimo woman, organized by the explorer Vilhjalmur Stefansson, had landed on Wrangel Island, Mr. Burnham had several

[3] John B. Burnham to D. C. Poole, February 6, 1922, DoS File 861.0144/1.

[4] Wrangel Island is located approximately eighty nautical miles to the north of the coast of Siberia, and about 160 nautical miles west of the longitude of the boundary meridian in Bering Strait between Alaskan and Russian territory.

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times visited the department to talk about the matter. Mr. Burnham believed that the United States was entitled to claim Wrangel Island. He also believed that the island was essential to American trade in North Pacific waters. He feared that unless steps were taken immediately all chances of the United States retaining Wrangel Island would be lost.

The point of Mr. Burnham's letter of February 6 was to inform the department that he had found out "the reason why Wrangell [*sic*] Island was taken possession of by the English last summer." He indicated that this was part of a plan to have the Hudson's Bay Company establish a trading monopoly in the general area of Siberia.

As a result of Mr. Burnham's letter and his visits to the department, A. W. Kliefoth of the Division of Russian Affairs undertook to make certain inquiries, in and out of the department, regarding the status of the island. His inquiries turned up the information that at various times Great Britain, Russia, and the United States had claimed the island, that on most maps it was assigned to Russia, and that the first person known to have actually landed on the island was an American officer, although before that others—among them Russians and Englishmen—had seen the island, had passed close by it, had sketched the outline of some of its coasts, and had given names to some of its bays. At the conclusion of his investigations, Mr. Kliefoth recommended that no action be taken until the ownership of the island could be definitely determined by the department.[5]

The department did not have occasion to take up the matter of Wrangel Island again until late March 1922. Then it did so as a consequence of the lead article on the front page of the *New York Times* for February 20, 1922, dealing with the expedition sent to Wrangel Island by Vilhjalmur Stefansson in the summer of 1921 and reporting that the purpose of that expedition had been to reassert British sovereignty over the island. It undertook to cover briefly the history of the island and gave Stefansson's version of the history of the various occupations of the island. Stefansson was reported as saying

[5] Memorandum, Division of Russian Affairs (A. W. Kliefoth) to the Chief of the Division of Russian Affairs (D. C. Poole), February 28, 1922, DoS File 861.0144/–; see also, letter, Department of the Interior to P. B. Gilbert, Chief of the Division of Political and Economic Information, March 3, 1922, DoS File 861.0144/2.

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that both the British and American governments had permitted their earlier claims to lapse by their failure to occupy the island continuously; that the last British claim—based on the fact that seventeen members of a Stefansson expedition, shipwrecked in the area in 1914, had occupied the island and raised the British flag—had also lapsed; and that it was to reassert the British claim to sovereignty over the island that the expedition of 1921 had been dispatched. The *Times* article went on to say that Mr. Stefansson had the previous week seen Canadian Prime Minister Mackenzie King for the express purpose of offering the island to Canada.

Immediately upon reading the article in the *Times,* Mr. D. C. Poole set in motion a series of inquiries into the status of Wrangel Island and American interests in that island that only ended—petered out might perhaps be the more appropriate way of putting it—in May 1924. In this process of examination, inquiries were made of various departments within the executive branch as to their possible interests in Wrangel Island; memoranda were written by the Solicitor's Office on the legal question of the acquisition of territory; researches were made into the history of the island; inquiries were sent to the American Embassy in London inquiring as to the possible British interest in laying claim to the island; discussion by intra-departmental memoranda was continued as to the appropriate American policy to be followed in light of the relevant data turned up by the previously mentioned activities. The results of these different lines of inquiry were in some respects conclusive, and in others much less so. The department never was able to obtain from the embassy in London a clear picture of British intentions; indeed, the embassy appears never to have received from the Foreign Office an answer to one *aide mémoire* it left with that office, even though the British did say that an answer would be forthcoming after information which it had asked for from the Dominion governments was in its hands. Nor was the department ever able satisfactorily to determine what its interests in the island were, and hence what its policy should be. For one thing, none of the departments within the executive branch can be said to have waxed enthusiastic over the possible value of the island to this country, although all of them did say the island might have some economic or

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military value. Interestingly enough, the Commerce Department wrote that the island might have military value, and suggested that the State Department might inquire further on this point of the War Department; and the War Department in its turn discussed the possible economic value of the island and suggested that State write a letter to Commerce.[6]

The first inquiry to the Solicitor's Office brought a quick response from Mr. Nielsen whose off-the-cuff opinion was that the United States did not appear to have much of a claim. Such claim as this country might have as a result of the activities of some of its naval officers, Mr. Nielsen felt might well have been jeopardized by the failure of this government to register objection to a notice which the Russian government had sent to the Department of State in 1916 asserting a claim to several Arctic Islands off the coast of Siberia, including Wrangel. Subsequent research by other members of the Solicitor's Office supported Mr. Nielsen's initial opinion, though these researches did advance the idea that the American claim was no weaker than that of several countries, notably Great Britain, Canada, Japan, and Russia, which had from time to time expressed an interest in the island.

Clearly, then, the first five years after the end of World War I were a time of relative quiet and unconcern for the Department of State so far as polar questions were concerned. Questions did arise from time to time, not only concerning Wrangel Island, but also concerning such minor matters as the status of the Arctic Island of Jan Mayen, occupied by the Norwegian government, but nothing seemed to require the close and serious attention of the department. The slow pace with which it conducted its inquiries into the Wrangel Island matter well indicates that the department felt little continuous pressure to reach some conclusions about its policy in polar areas.

III

Then in January 1924 there occurred an event the im-

[6] A summary of the replies received from the departments in the executive branch to a letter sent to them by D. C. Poole, is given in a memorandum written by Mr. Poole, DoS File 861.0144/53.

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mediate outcome and impact of which could not possibly have been predicted at the time it occurred. On January 12, 1924, the Secretary of the Navy, Edwin Denby, appeared to testify before a subcommittee of the House Appropriations Committee on a pending appropriations bill. During the course of his testimony he remarked, by way of comment on the proposed polar flight of the Navy airship *Shenandoah,* that one of the purposes the flight might achieve was the discovery of new territory in the more than one million miles of unexplored area between Nome, Alaska, and the North Pole. The Secretary added that it was "highly desirable that if there were any undiscovered country in that region [between Nome and the North Pole] it should be seized by the United States at as early a date as possible." He later added:

> We have to photograph those regions and plant our flag there first. . . . But for myself it is also a matter of great national pride that we shall not let any other nation step in between us and this region, which is practically contiguous to the territory of the United States; and that is exactly what some nation will do if we do not get there first.[7]

Not surprisingly, perhaps, the Secretary's statements to the committee were picked up and carried in newspapers both in this country and in Canada. In Canada, needless to say, Secretary Denby's statements brought a mixed press reaction. Some newspapers professed an inability to know whether to take Mr. Denby seriously. Others assumed that Mr. Denby, with his vision of better than one million miles of unexplored territory "practically contiguous" to the United States, was simply indulging in the American pastime of "spread-eagling" and could not, of course, be taken seriously; they chose to be annoyed by his statement anyway.[8]

This might have been the end of the matter had not Secretary Denby's statement also been noticed by Anson W.

[7] Mr. Denby's testimony, and comments on it in newspapers, are given in a memorandum written by A. B. Haupt, Office of the Solicitor, dated March 22, 1924, DoS File 857.014/9.

[8] Secretary Denby's testimony was reported on the front page of the *New York Times* for Sunday, January 20, 1924, under a headline reading: "Annexation of Land to [North] Pole, if Any, Aim of Navy, says Denby." For a report of comments in the Canadian press, see, dispatch, unnumbered, The Consul General, Montreal, to the Secretary, January 22, 1924, DoS File No. 811.014/98.

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Prescott, whose official position was secretary of the Republican Publicity Association. Undoubtedly seeing some potential publicity of value to the incumbent Republican administration in this item of news, Mr. Prescott wrote a letter on January 21 to Secretary Denby asking generally for information regarding the proposed flight of the *Shenandoah* and particularly concerning "the present status of Wilkes Land." Secretary Denby replied to Mr. Prescott with a letter dated January 31, 1924, in which he reviewed briefly past voyages to the Antarctic, including that of Lieutenant Wilkes in 1838–40, and the claims which had been asserted to territory in that region. But as to the question of a possible claim by the United States the Secretary declined to comment, saying that such matters properly fell within the province of the Department of State.[9]

Upon receiving Secretary Denby's reply, Mr. Prescott sent off a letter almost immediately to the Secretary of State requesting information on the status of Wilkes Land. With his letter he enclosed a copy of the letter he had received from Secretary Denby.[10]

Mr. Prescott's letter arrived in the Index Bureau of the Department of State on February 4, 1924, went two days later to the Solicitor's Office, and thence to the Division of Economic and Political Information, where a draft reply was prepared by Colonel Lawrence Martin. Before this, or any, reply could be got off, a second letter, dated February 16, 1924, arrived from Mr. Prescott. This letter, quite brief, asked if the department would indicate whether it had received Mr. Prescott's letter of February 2, and if so, whether the Republican Publicity Association might expect to receive a reply.[11]

This second letter from Mr. Prescott went first to the Index Bureau (where it was stamped February 18, 1924) and immediately to the Solicitor's Office (where it received a date stamp of February 20, 1924) and then to the Division of Political and Economic Information. Still, for reasons which the department's archives do not make obvious, no

9 The Secretary of the Navy to A. W. Prescott enclosure to letter, A. W. Prescott to the Secretary, February 2, 1924, DoS File 811.014/99.

10 A. W. Prescott to the Secretary, February 2, 1924, DoS File 811.014/99.

11 A. W. Prescott to the Secretary, February 16, 1924, DoS File 811.014/101.

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answer went out to Mr. Prescott, and another three weeks passed before any further action on this matter was taken.

IV

In the meantime, there came to the department from the Norwegian Minister a note, dated February 25, concerning a report published in the *Rochester* [New York] *Herald* about the polar flight of Roald Amundsen. This report said, among other things, that the Norwegian explorer Amundsen had agreed that the United States might claim all the land he might find on the expedition he was planning to make to the north polar regions. This assertion by Mr. Amundsen became known, the newspaper report said, from a letter in the files of the U. S. Navy Department signed by a Mr. H. H. Hammer, the American representative of Mr. Amundsen. The report also said that Secretary Denby had acknowledged Mr. Amundsen's offer with thanks and had agreed to lend an American sailor, Lieutenant Ralph Eugene Davison, to the expedition. The Norwegian Minister went on to say in his note that upon learning of what was reported in the *Rochester Herald,* Mr. Amundsen stated to the Norwegian Minister of Foreign Affairs in Christiania that he had in no way given his consent to the annexation of land for the benefit of the United States. The Minister then said by way of concluding his note: "In order to avoid any misunderstanding, I beg to add that possession of all the land that Mr. Amundsen may discover will, of course, be taken in the name of His Majesty the King of Norway." [12]

Immediately upon receiving the Norwegian Minister's note, Mr. Leland Harrison, Assistant Secretary of State, sent a copy of it to the Secretary of the Navy with a letter in which Mr. Harrison asked for such comments as the Secretary of the Navy might wish to make.[13]

While the Navy's answer was awaited, the question of answering Mr. Prescott's earlier letters arose once more, this time through a memorandum dated March 13, 1924, written by Colonel Lawrence Martin of the Division of Political and

[12] The Norwegian Minister to the Secretary, February 25, 1924, DoS File 857.014/5.

[13] The Assistant Secretary to the Secretary of the Navy, February 28, 1924, DoS File 857.014/5.

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Economic Information, and sent by him circular fashion to the Solicitor, the Division of Western European Affairs, the Economic Adviser, and the Second Assistant Secretary.[14]

Colonel Martin's memorandum was typical in several respects of memoranda written during this period dealing with polar regions. It was typical in that it invited discussion of what course the United States should follow; policy for Antarctica was still regarded as open to decision. It was also typical in that it hinted at the potentialities of Antarctica, both economic and military. No one in the department wanted the United States to miss out on a possible good thing, and one of the aspects of Antarctica that kept it before the minds of the State Department people in this period was its future possible benefits. Then, too, the memorandum was typical in that it suggested that time might be running out, that the British were moving into the area fast, and that soon it might be too late for the United States to assert the claims it might legitimately have. Finally, it was typical in repeating a proposal, discussed before, that the United States take the lead in proposing a treaty along the lines of the Spitzbergen Treaty under which all countries that had shown interest in the development of Antarctica would be given guaranteed access to that region.

While Colonel Martin's memorandum was making its way around the department, the department received from the Navy the letter it had been waiting for. Now the department could proceed to draft a reply to the note of February 25, 1924, from the Norwegian Minister. The Navy's letter made three points: first, that the Secretary of the Navy had never acknowledged with thanks an offer from either Mr. Amundsen or his American representative, Mr. Hammer, to allow the United States to claim any territory that might be discovered on the Amundsen polar expedition; second, that the only letter that the Secretary of the Navy had written to Mr. Hammer was one in which the Secretary refused Mr. Hammer's request to fly as a passenger from Germany to the United States in a Navy plane; and third, that Lieutenant Davison had volunteered and had requested that he be per-

[14] Memorandum, the Division of Political and Economic Information to the Solicitor, the Division of Western European Affairs, the Office of the Economic Adviser, and the Second Assistant Secretary, March 13, 1924, DoS File 811.014/101.

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mitted to accompany Captain Amundsen on his expedition, that Lieutenant Davison had been granted permission to do so, and finally, that the Navy had no official connection with or any control over the plans of Captain Amundsen to carry it forward.[15]

V

With this information from the Navy on hand, the writing of a reply to the Norwegian Minister went forward. The first draft reply prepared and sent around through the department for initialing was a brief answer of two paragraphs which did nothing more than transmit to the Minister the substance of the Navy's letter of March 14 to the department. Just as that reply was ready to be dispatched, the Under Secretary, Mr. Phillips, had second thoughts about it, and sent it to the Solicitor with this memorandum:

> You will note that in this draft reply to the Norwegian Minister, we completely ignore the statement in the concluding paragraph of his note, in which he serves notice that 'possession of all the land that Mr. Amundsen may discover will, of course, be taken in the name of His Majesty the King of Norway.' In view of the possible importance of this matter in the future and the tacit admission which we are giving by accepting this statement without comment, I should feel happier to see your initials.[16]

Mr. Phillips' memorandum to Mr. Hyde resulted in the writing of two additional memoranda, one by Colonel Martin, dated March 21, 1924, and the other by A. B. Haupt of the Solicitor's Office.

Colonel Martin wrote his memorandum because A. B. Haupt had asked him what he thought should be said in the department's reply to the Norwegian Minister to take care of the problem raised by Mr. Phillips. In suggesting words he thought would cover the problem, Colonel Martin used the opportunity once more to recommend that the United States try to get a treaty—this time for the Arctic regions—modelled after the Spitzbergen Treaty. He suggested that

[15] The Acting Secretary of the Navy to the Secretary, March 14, 1924, DoS File 857.014/6.

[16] Memorandum, the Under Secretary to the Solicitor, March 19, 1924, DoS File 857.014/5.

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for the moment the department should put off answering the Norwegian Minister's note until he could make a study of Arctic exploration by Americans. "What I have in mind," said the Colonel,

> is to show that the United States has no valid claim to the Arctic Archipelago west of Greenland and north to Canada; that Norway has no such claim, though Sverdrup and Amundsen asserted one to certain islands discovered by them; and that Great Britain has no such claim, though she has asserted one since 1920 which overlaps the American and Norwegian discoveries.
>
> The natural conclusion is that the Arctic Archipelago, together with any islands which may be discovered north of Alaska and Canada, should be regarded as *Terrae Nullius*. An obvious suggestion is that the Department should consider proposing to Norway and Great Britain a joint Convention, similar to the Spitzbergen Treaty which we have just ratified, by which American, British and Norwegian ships and nationals should enjoy equal rights of fishing, hunting, mining, commerce, et cetera upon the lands and islands of the Arctic Archipelago and within their territorial waters (if a *terra nullius* has territorial waters).[17]

The memorandum A. B. Haupt prepared was a lengthy one, dealing with one legal question which Mr. Haupt put as follows: "Whether discovery from an aeroplane of land in the Arctic region gives a right of possession in the land so discovered to the country of the national making the discovery." Mr. Haupt's answer was an unqualified "No" which he buttressed by citation of the appropriate international legal authorities, Hall, Oppenheim, and Hyde. He agreed with Colonel Martin's suggestion that the United States should inform the Norwegian Minister that it could not admit the contention of that government that title to any land that might be discovered by Mr. Amundsen could be taken in the name of the government of Norway. In this way, Mr. Haupt said, the United States would reserve all its rights in the matter. It did not, therefore, seem to him necessary to put off replying to the Norwegian Minister until the department had a chance to consider Colonel Martin's

[17] Memorandum, Division of Political and Economic Information to the Solicitor, March 21, 1924, DoS File 857.014/7.

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proposal that discussions be opened with Great Britain, Norway, and other interested countries on the permanent status of the polar regions. This matter, said Mr. Haupt, might be considered by the department at its convenience.[18]

Mr. Haupt's arguments proved persuasive, and a reply, incorporating the following paragraph written by Solicitor Hyde, was sent to the Norwegian Minister on April 2, 1924:

> In the penultimate paragraph of your letter you state that, in order to avoid any misunderstanding, you would add that possession of all the land which Mr. Amundsen may discover will, of course, be taken in the name of His Majesty, the King of Norway. In my opinion rights similar to those which in earlier centuries were based upon the acts of a discoverer, followed by occupation or settlement consummated at long and uncertain periods thereafter, are not capable of being acquired at the present time. Today, if an explorer is able to ascertain the existence of lands still unknown to civilization, his act of so-called discovery, coupled with a formal taking of possession would have no significance, save as he might herald the advent of the settler; and where for climatic or other reasons actual settlement would be an impossibility, as in the case of the Polar regions, such conduct on his part would afford frail support for a reasonable claim of sovereignty. I am therefore compelled to state, without now adverting to other considerations, that this Government cannot admit that such taking of possession as a discoverer by Mr. Amundsen of areas explored by him could establish the basis of rights of sovereignty in the Polar regions, to which, it is understood, he is about to depart.[19]

With the sending of this note to the Norwegian Minister, containing the paragraph written by Mr. Hyde, the department was now specifically committed, for the first time in its history, to a general position respecting the acquisition of territory in polar regions.

VI

It might seem that the department could now have gone ahead immediately to answer the long unanswered Prescott

[18] Memorandum, Office of the Solicitor, March 22, 1924, DoS File 857.014/9.

[19] Note, the Secretary to the Norwegian Minister, April 2, 1924, DoS File 857.014/6.

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letters. But this did not happen. Almost a month elapsed before the matter of the Prescott letters was raised. In late April, Mr. Stanley K. Hornbeck, of the Economic Adviser's Office, wrote a memorandum to the Second Assistant Secretary, Mr. Adee, in which he noted that no action had been taken on the Prescott letters despite the fact that Colonel Martin had expressed the hope in a memorandum written on March 13 that the matter might be attended to immediately.[20] Mr. Hornbeck's note finally secured action on the Prescott letters, and on May 13, 1924, a lengthy letter was sent to Mr. Prescott. It followed closely the lines of the draft letter prepared by Colonel Martin in early March. The most important paragraphs of this letter—which has since that time been known as the expression of the "classic United States position" on the acquisition of territory in polar regions—read as follows:

> Sir:
>
> The receipt is acknowledged of your letters dated February 2, and February 16, 1924, in which you ask whether the United States has a valid claim to Wilkes Land, Antarctica, by right of discovery, whether that claim has ever been proclaimed, and if not, what the objections may be, in law or policy, to annexing the territory to this country. . . .
>
> So far as this Department is informed the exploration of parts of the Antarctic Continent by American, Belgian, British, French, German, Norwegian, Russian, Swedish, and other travelers has not been followed by permanent settlement upon any part of the continent.
>
> It is the understanding of this Department that the so-called Ross Dependency of New Zealand has no permanent population. This part of the Antarctic Continent lies immediately east of Wilkes Land.
>
> It is likewise the understanding of this Department that the portion of the so-called Dependency of the Falkland Islands upon the mainland of the Antarctic Continent, which is referred to as 'Graham's Land' in the British Letters Patent of July 21, 1908, and of March 28, 1917, and which was discovered by Captain N. B. Palmer in 1820–21 and named Palmer Land by the United States Geographic

[20] Memorandum, Office of the Economic Adviser, to the Second Assistant Secretary, April 29, 1924, DoS File 811.014/101.

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> Board on November 6, 1912, has no permanent population, although it is understood that British and other whalers use the shoal waters near shore in summer for anchorage.
>
> It is the opinion of the Department that the discovery of lands unknown to civilization, even when coupled with a formal taking of possession, does not support a valid claim of sovereignty unless the discovery is followed by an actual settlement of the discovered country. In the absence of an act of Congress assertative in a domestic sense of dominion over Wilkes Land this Department would be reluctant to declare that the United States possessed a right of sovereignty over that territory.[21]

On the writing and contents of this now famous letter a few remarks are in order. First, as was noted earlier,[22] the letter was primarily the work of Colonel Lawrence Martin of the Division of Political and Economic Information. Apart from only one change, a minor matter of phrasing suggested by Solicitor Hyde, the letter that finally went to Mr. Prescott was one which had been ready since early March.

The second comment that may be made is that the letter was widely circulated through the department for approval by the chiefs of the several divisions. Among those who initialed it were Assistant Secretary Leland Harrison, Second Assistant Secretary Alvey A. Adee, the Solicitor, Mr. Hyde, and officers of the Division of Western European Affairs, the Latin American Division, and the Office of the Economic Adviser. This rather impressive collection of initials suggests that although the department might have taken its time about getting to it, this was nonetheless a matter which the department thought was of sufficient interest to warrant getting the approval of several divisions before the letter was dispatched.

The third point one should note about the letter is that it contained no suggestion that the department favored some kind of internationalization, resembling that accorded the Island of Spitzbergen, for example, for polar lands. This idea had had wide discussion within the department in this period and several members favored it, but no mention of it appears in the Prescott letter itself. That the department fa-

[21] The Secretary to A. W. Prescott, May 13, 1924, DoS File 811.014/101.

[22] See *supra* p. 83.

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vored such a development might, perhaps, have been read between the lines of the letter, particularly in the light of the suggestion made there that occupation (which might reasonably have been thought to be impossible given the climatic conditions of the polar areas) was the only way by which any one nation might make its claim to polar lands effective. This is what the draftsman of the letter had in mind, and it seems likely that it was also in the mind of Secretary of State Hughes. In a memorandum he wrote in 1926, Mr. Prentiss Gilbert said that Mr. Hughes believed that the unusual climatic conditions prevalent in the polar regions, creating as they did special problems in the matter of acquiring sovereignty, required an international policy for those areas, one provided by an appropriate treaty perhaps.[23]

VII

So much for the circumstances under which the Prescott letter was written. One must now ask for what purpose that letter was designed.

The commentators have suggested various purposes, some that the letter was intended to be an answer to those who had for some time been urging that the United States stake its claim to a share of the territory of Antarctica,[24] others that it was intended to serve notice that the United States could not concede that the sector principle could provide a basis for a valid claim to territory in the Antarctic.[25]

The analysis of the department's handling of polar questions given in the earlier pages of this study suggests that both of these explanations fall somewhat short of the mark. Both suffer from the error of assuming that the Department of State had elected to take the position it did in the Prescott letter because the department had concluded—presumably only after careful and considered deliberation and weighing of alternatives—that that position was the one best able to

23 Memorandum, Prentiss Gilbert, Division of Western European Affairs, to W. R. Castle, June 11, 1926, DoS File 857.014/30.

24 Walter Sullivan, "Antarctica in a Two-Power World," *Foreign Affairs*, XXXVI (1957), 155.

25 Robert D. Hayton, "The 'American' Antarctic," *American Journal of International Law*, L (1956), 585.

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serve the known interests of the United States. But the reconstruction of the events that led up to the writing of the Prescott letter leaves a contrary impression; it suggests that up to the time of the writing of the Prescott letter, the department had been unable to determine what its interests in polar regions were, and hence had been unable to decide what policy or course of action would best promote those interests.

If this is so, then one is compelled to explain why the Prescott letter was written at all, why the department did not wait to reply to Mr. Prescott until it had completed its consideration of this problem and reached some firm conclusions as to what this country's interests in polar regions were and what policy and course of action would be best designed to promote those interests. The answer to this question would seem to hinge in part on the person of Mr. Prescott himself. He was, after all, no less than the secretary of the Republican Publicity Association, and letters from him presumably could not be ignored. Ideally, perhaps, letters from him would be answered promptly. But this did not happen in the case of Mr. Prescott's letter of February 2. Not only was it not promptly answered, not even a letter simply acknowledging the receipt of it was sent, even after Mr. Prescott had specifically asked the department, in his letter of February 16, if the department could let him know when he might expect an answer to his letter of February 2. And this omission, as Miss Hanna of the Bureau of Coordination and Review pointed out, was a violation of established department routine. It was April before Mr. Hornbeck was finally successful in moving the department to take up in earnest the matter of a reply to Mr. Prescott's letters, even though it would seem that the reply which ultimately did go to Mr. Prescott was available in draft form not later than early March. By the beginning of May, in brief, there could hardly have been any doubt in the mind of anyone in the department that some answer had to go to Mr. Prescott, and the sooner the better.

There arose then the question of what to say to Mr. Prescott. Happily this was not a considerable problem, for the department had available to use as a guideline both the draft reply to Mr. Prescott which Colonel Martin had written in March, and the answer that had been made to the

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Norwegian Minister in the department's note to the Minister dated April 2, 1924. Just as happily, the two documents had been drafted by Colonel Martin, and not surprisingly, the position taken in them was very close. There was only one obvious but minor difference between them. The note to the Norwegian Minister admitted the "impossibility" of actual settlement of the polar regions, whereas the Prescott letter omitted any reference to this point. All other differences between them were matters of phrasing. The note to the Norwegian Minister asserted that mere discovery even when coupled with formal taking of possession could give but "frail support for a reasonable claim of sovereignty"; the Prescott letter put the same idea in a somewhat more positive form by asserting that "discovery of lands unknown to civilization, even when coupled with a formal taking of possession, [did] not support a valid claim of sovereignty unless the discovery [was] followed by an actual settlement of the discovered country."

If this explanation of the origin and purpose of the Prescott letter be correct—and many memoranda support it while none contradict—then it may be seen that the Prescott letter, far from being designed as a statement of the department's considered position as of May 1924 on the acquisition of territory in polar regions, was rather designed as a stop-gap measure.

As such the letter filled two major needs of the Department of State at the time: it attempted to protect, by a reassertion of the then-existing rules of international law, any rights which the United States might subsequently find she was entitled to assert in polar regions, and which she might conclude her interests required her to assert; at the same time it gave the department time to undertake a thorough consideration of the problem presented by polar regions, specifically to determine whether the peculiar character of those regions did not demand that all nations interested in them come together to develop a new policy that was appropriate to those conditions. Additionally, the letter had what one may call a "bonus feature." It permitted the department to send Mr. Prescott's letters of February 2 and 16 to the files marked "Answered, 5/13/24."

What is perhaps most interesting about the Prescott letter

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is that in taking the position it did—reasserting the then-existing rules of international law on the acquisition of territory—both the motive of the department in writing the letter and its thought at that time on the acquisition of territory in polar regions were badly obscured. To any but a member of the department at the time the Prescott letter was written, it must have appeared that the department—presumably only after careful consideration of the matter—had taken this opportunity to comé out firmly in support of the then-existing rules of international law.

But various memoranda, portions of which have been cited, make it clear that such was hardly the case. These memoranda indicate instead that the department was not at all certain that existing rules of international law were appropriate to the peculiar conditions of the polar regions, particularly in light of the fact that those territories were uninhabitable; and that accordingly the Prescott letter was designed to serve essentially as a stop-gap measure. The position of the Prescott letter, in sum, was not intended to be a final or even necessarily a long-range policy. Yet the close examination into American interests in the polar regions and the consequent design of a policy calculated to serve those interests, which one might have expected to follow immediately after the writing of the Prescott letter, did not occur in the nineteen-twenties.

This failure to reconsider the policy of the Prescott letter did not, however, go unnoticed. Quite regularly during the nineteen-twenties persons within the department raised the question of the soundness of leaving the policy of the Prescott letter unrepudiated. The department was said to lack a settled policy; [26] the department's attitude as expressed in the Prescott letter was described as "inert" and "complacent"; [27] Mr. Prentiss Gilbert once expressed the hope that the department would not take a "dog-in-the-manger" attitude toward the acquisition of territory in the Antarctic, but the very context of his suggestion made it clear that Mr. Gilbert thought that the department had already come danger-

[26] Memorandum, Division of Western European Affairs (I. N. Linnell), July 13, 1925, DoS File 800.014 Arctic/8.

[27] Memorandum, Division of Western European Affairs (R. L. Keiser), August 6, 1926, DoS File 800.014 Arctic/16.

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ously close to taking just such an attitude.[28] On still other occasions, Mr. Linnell and Mr. Hyde [29] pointed out a troublesome conflict between the Prescott letter and the Monroe Doctrine, and suggested that it was time to resume consideration of American policy toward Antarctica.

Despite all this vigorous criticism of the policy of the Prescott letter, a reconsideration of that policy did not occur. Thus, by default, the Prescott letter became something it was never intended to be: the only public statement of American policy toward the acquisition of territory in the Antarctic region. And as each day passed without any visible challenge to the position it represented, that position gained greater authority and credence as the official United States position on the acquisition of sovereignty over polar areas.

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MILLSAPS COLLEGE

[28] Memorandum, Division of Western European Affairs (Prentiss B. Gilbert to W. R. Castle) June 11, 1926, DoS File 857.014/30.

[29] Memorandum, Division of Western European Affairs (I. N. Linnell) July 13, 1925, DoS File 800.014 Arctic/8; memorandum, Office of the Solicitor (C. C. Hyde) to the Secretary, June 18, 1925, DoS File 800.014 Arctic/6.

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THE POLITICAL BEHAVIOR APPROACH: METHODOLOGICAL ADVANCE OR NEW FORMALISM?—A REVIEW ARTICLE

ON THE frontiers of political science research, the dominant fashion is now the political behavior approach. In the words of one of its most respected practitioners:

> Historically speaking, the behavioral approach was a protest movement within political science. Through usage by partisans, partly as an epithet, terms like political behavior and the behavioral approach came to be associated with a number of political scientists, mainly Americans, who shared a strong sense of dissatisfaction with the achievements of conventional political science, particularly through historical, philosophical, and descriptive-institutional approaches, and a belief that additional methods and approaches either existed or could be developed that would help to provide political science with empirical propositions and theories of a systematic sort, tested by closer, more direct and more vigorously controlled observations of political events.[1]

It is important to understand that "political behavior" does not refer to any particular body of knowledge but is rather an orientation applicable to the study of all aspects of government and politics:

> To treat [political behavior] as a 'field' coordinate with (and presumably isolated from) public law, state and local government, international relations, and so on, would be to defeat its major aim. That aim includes a reworking and extension of most of the conventional 'fields' of political science.[2]

Perhaps the most characteristic recent feature of the behavioral approach has been its great stress on the need for polit-

[1] Robert A. Dahl, "The Behavioral Approach in Political Science: Epitaph for a Monument to a Successful Protest," *American Political Science Review*, LV (1961), 766.

[2] David B. Truman, "The Implications of Political Behavior Research," Social Science Research Council, *Items*, V (1951), 37.

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ical science research to become more systematic. This has meant, on the one hand, that the research should take place within an explicitly stated "conceptual framework" setting forth "the organizing ideas and concepts which, expressed in rigorous and orderly statements of relationships between variables, provide the basis for deriving hypotheses about behavior that can be tested empirically." It has meant, on the other hand, that the research should use the purportedly more "scientific" concepts and the more quantitative data-collecting techniques of sociology, social psychology, and cultural anthropology wherever possible and relevant.[3]

As the behavior(al)ists [4] themselves realize, full acceptance of their approach by the more traditionally minded in the discipline must await fuller demonstration that it yields "explanations of some important aspects of politics that are more thoroughly verified, less open to methodological objections, richer in implications for further explanation, and more useful in meeting the perennial problems of political life than the explanations they are intended to replace." [5] Three imposing volumes have been published within the past year that, in various respects, exemplify the political behavior approach.[6] It might be instructive to subject them to critical review to see whether they are contributing to that demonstration.

II

In *Political Ideology* Robert Lane attempts to specify "the

3 Avery Leiserson, "Problems of Methodology in Political Research," *Political Science Quarterly*, LXVIII (1953), 569; Samuel J. Eldersveld, *et al.*, "Research in Political Behavior," *American Political Science Review*, XLVI (1952), 1003–09.

4 For the merits of "behavioralism" as the most correct designation of the approach, see David Easton, "Introduction: The Current Meaning of 'Behavioralism' in Political Science," in James C. Charlesworth (ed.), *The Limits of Behavioralism in Political Science* (Philadelphia, 1962).

5 Dahl, 768.

6 *Political Ideology: Why the American Common Man Believes What He Does.* By Robert E. Lane. New York and London, The Free Press of Glencoe, 1962.—xi, 509 pp. $10.00; *The American Polity: A Social and Cultural Interpretation.* By William C. Mitchell. New York and London, The Free Press of Glencoe, 1962.—xiv, 434 pp. $10.00; *The Legislative System: Explorations in Legislative Behavior.* By John C. Wahlke, Heinz Eulau, William Buchanan, and Leroy Ferguson. New York and London, John Wiley and Sons, Inc., 1962.—xii, 517 pp. $8.95.

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latent political ideology of the American urban common man," "the sources of this ideology in the culture and experiences this common man knows," and the way in which this ideology "suports or weakens the institutions of democracy." The research consisted of a series of extended, depth interviews with a random sample of fifteen working- and lower-middle-class men from "Eastport, U.S.A., a city of over 100,000 population in the Atlantic seaboard area." The conversations with the men were recorded and transcribed, and the material in 3,750 pages of verbatim transcript sorted, coded, classified, and analyzed.

Lane's findings are of some comfort to an American democrat. If the findings are at all representative, at least the "successful" urban common man is not part of an undifferentiated mass society, seeking to escape from freedom, but is a

> conservative in the sense that he has no program for structural change in society, is markedly loyal to the prevailing system of government, believes in private property and capitalism, holds that he is living in a moral order where people get pretty much the rewards and punishments that they merit, and assumes a general responsibility for his own fate. . . . These men are not alienated . . . they believe that the government is generally concerned about their welfare.[7]

Furthermore, the preponderant majority of the sample exhibited personalities with enough confidence to tolerate the "delay, confusion, and ambiguity" that go along with the democratic political process, and able to share power, neither "subordinating themselves uncritically nor rebelling against the authority of others" nor "seeking exclusive power for themselves."

Lane finds the source of this ideology in the men's work experiences, their experience with money, their early family background, and their participation in voluntary organizations. He concludes that the basic support for the democratic system derives from its being perceived as part of the general society which provides, on the whole, a satisfying life and which more specifically allowed the interviewees to improve their social and economic positions relative to those of their fathers. Lane sees a second important basis of support

[7] Lane, 250–51, 130.

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in the experiences the men have had with democratic procedures in small groups, such as their housing development's "Community Council," American Legion posts, lodges and unions, and PTA's.

Lest it be thought that Lane's book puts to rest all previous worries about the vulnerability of American democracy, it should be noted that although the common man says unabashedly that he is in favor of "freedom," the chief content he assigns to the term "freedom" is freedom in the choice of some particular religion and freedom of personal movement, not the essentially political freedoms:

> One might search in vain in the streets of Eastport for men familiar with the provisions of the Constitution protecting freedom of speech, freedom of assembly, freedom to petition for the redress of grievances. There is little knowledge of the rudiments of due process; . . . men willing to sacrifice for the rights of a Communist for a fair trial, or atheists for a fair hearing . . . would be hard to find. . . .[8]

Secondly, neither does the common man value what has come to be regarded by some as another essential part of American democracy—equality of opportunity. In what may be the most controversial of Lane's conclusions, he argues that many members of the working class not only do not want equality but that they are even afraid of it: "Equality for the working classes . . . is a worrisome, partially rejected, by-product of the demand for more specific measures. Inequality has values that have been overlooked." [9]

Thirdly, a fifth of Lane's interviewees were indeed "undemocrats," having destructive personalities fundamentally opposed to popular government.

Lately in the United States a number of influential writers have been warning that our political institutions are in danger; they argue that lack of community feeling and of an integrated and coherent belief system is creating widespread malaise, anomie, and alienation from the society and its political system. Whether or not they are correct is of some importance to the future of American democracy. In this important study Lane musters direct, empirical evidence which suggests that they are not correct, at least with respect

[8] Lane, 461.

[9] Lane, 60.

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to one large segment of the population. Furthermore, Lane argues very convincingly in a concluding theoretical section that the very patterns of the *Gemeinschaft* way of life for which these writers seem to be calling would entail consequences tending rather to weaken than to strengthen a democratic order such as our own.

III

William Mitchell's *The American Polity* is an "*interpretative essay* on American politics as viewed in the early 1960's," guided "by an explicit conceptual framework derived mostly from the sociological work of Talcott Parsons." Mitchell's two major objectives are "to illustrate the potentialities of 'structural-functional' analysis with regard to the study of political systems" and "to synthesize a considerable amount of heterogeneous data—both institutional and behavioral—on American politics so that we might have a more integrated view of our nation's political life."

In the best practice of the behavioral approach, Mitchell's first chapter presents a "conceptual framework" which is based on the notion of a "social system," of which the "polity" or political system is a subsystem. As the author conceives it, the polity receives "inputs" of various "demands," "resources," and "support" from other subsystems of the society and furnishes certain "outputs" among which the four most important are "the authoritative specification of system goals," "the authoritative mobilization of resources to implement goals," "the integration of the system," and "the allocation of values and costs." The performance of these functions is affected by the "internal structure" of the polity and by the "political culture" within which it operates. Finally, the system's persistence over time is seen as an example of "equilibrium" with a provision also being made for the inevitability of "political change."

The main body of Mitchell's book is organized around the categories of this conceptual framework. There are sections describing the structure of the American polity, its distribution of power, its belief system, the processes by which its central functions are performed, and some past and current trends in its politics.

There is a real need for a masterful integration of the

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empirical data that have been collected over the past decade in order to further our understanding of how the American political system works as a whole. Unfortunately *The American Polity* does little to fill it, its purported integration even being misleading in some respects. For example, great stress in the analysis is placed on the "different strata" in the United States tending "to hold in common similar expectations of political performance and similar general political beliefs such as the belief in democracy, majority rule, checks and balances, federalism, or the structure of our polity and government . . . political beliefs are widely shared regardless of class." [10] Not only is there no data supporting all parts of Mitchell's statement, but it also misleads in not explicitly making the point demonstrated by Lane and argued in a recent summary of the survey data by V. O. Key [11] that whatever widespread agreement exists is only on the highest level of generality. All this causes Mitchell to overestimate both the actual extent and the necessity of detailed, mass public support for our democratic order. Similarly, the specific content that Mitchell assigns to other parts of the "American belief system," however plausible at times, is again only very lightly based on evidence, and his statement that "Americans . . . find it hard to think of politics without using moral terms and symbols" [12] again conflicts sharply with Lane's empirically based findings of the "unmoralistic tendencies of the American common man." [13] Secondly, when Mitchell criticizes the "pluralistic group" model of power, his description of David Truman's "shortcomings" fit somewhat loosely with the actual statements in the latter's *The Governmental Process*.[14] Finally, Mitchell's assertion that "Today, according to the research of sociologists such as Floyd Hunter, we know that economic enterprises exercise a preponderance of political control over the decisions of many large cities, as in the case of Atlanta, Georgia," [15] simply ignores the serious

[10] Mitchell, 45.

[11] V. O. Key, Jr., *Public Opinion and American Democracy* (New York, 1961), 135–38, 550.

[12] Mitchell, 118.

[13] Lane, 344.

[14] Compare, for example, statements in Mitchell, 84 and 86 with those in Truman, 350–51 and viii.

[15] Mitchell, 182.

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limitations that have been pointed out in Hunter's methodology and substantive findings in articles cited earlier in Mitchell's own book.[16]

Thus when providing analysis or description on a relatively high level of specificity, Mitchell's assertions are often unsupported by specific evidence and occasionally conflict with, rather than integrate, the existing literature even when the latter is cited in his footnotes. On the other hand, when operating on a high level of generality or abstraction, Mitchell's functional analysis sometimes confuses and retards rather than promotes understanding.

"Structural-functional analysis" is a technical term for the relatively simple idea that social systems are made up of a number of identifiable parts, that those parts interact in certain ways, and that a fruitful method of explaining the system is to describe the parts—the "structures"—and explain the significant consequences—the "functions"—of each part for the others and the way in which their interactions affect the character of the system as a whole.[17] Much political science writing on the roles of parties, interest groups, public opinion, and the like is implicit functional analysis. Mitchell's chief theoretical difficulty is that by choosing the political system as a whole as his structural unit of analysis, he is forced to specify the functions and processes of the polity in such highly abstract terms that their connection with the "real-life" data that he is trying to integrate often becomes highly tenuous. Furthermore, by failing explicitly to differentiate the political system from one of its constituent parts—the government—Mitchell at times reifies the former concept, as when he has it "authoritatively specifying and implementing" "societal" or "social" goals where the context suggests that what he actually has in mind is the government adopting and executing various policies and programs. Not only would the latter terminology have saved Mitchell from

[16] See Nelson W. Polsby, "Three Problems in the Analysis of Community Power," *American Sociological Review*, XXIV (1959), 796–803 and Raymond E. Wolfinger, "Reputation and Reality in the Study of 'Community Power,'" *ibid.*, XXV (1960), 636–44, cited in Mitchell, 409, n.7.

[17] See Robert K. Merton, *Social Theory and Social Structure* (rev. ed.; Glencoe, Ill., 1957), 19–84; Bernard Barber, "Structural-Functional Analysis: Problems and Misunderstandings," *American Sociological Review*, XXI (1956), 129–35.

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having to talk about "societal goals" that are "less than widely approved," but it also would have pointed to the relations between the actual goals and interests of members of a society and the policies of their government as a significant question for analysis rather than passing it over in terminological obfuscation.

IV

The Legislative System by John Wahlke, Heinz Eulau, William Buchanan, and Leroy Ferguson is a comparative study of four American state legislatures—those of California, New Jersey, Ohio, and Tennessee—whose principal objective was to discover what kind of behavior their members thought was appropriate in carrying out their various duties as legislators. In more technical terms this was an exploration of legislative role-orientations, with the four bodies examined being considered in some respects as representative of American legislatures and, in a still more limited sense, of legislatures generally.

Like Mitchell's volume this study begins with a chapter explicitly setting forth the "theoretical framework" within which the research was undertaken (as well as a second chapter outlining the methodology). The authors' major working hypothesis was that the sociological and social psychological concept of "role" is the most fruitful unit of analysis in gaining knowledge "about the generic problems of legislative institutions and processes in American state government, and of similar institutions and processes . . . in other political systems."

The total role of the legislator was conceived as made up of three "sectors," each consisting of those subsets of norms involved in interacting with various categories of persons or positions with which a legislator has regular dealings, such as other legislators generally (the core-roles sector), legislators who occupy certain specialized statuses (specialized-subroles sector), and a variety of extra-legislative political actors like political parties, constituencies, interest groups, and executive branch officials (clientele-roles sector). The research consisted of systematically interviewing substantially all of the members of the 1957 sessions of the legislatures involved with an interview schedule aimed at discovering the major

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patterns of role concepts held in each of the sectors referred to above. The legislators' responses (to mainly open-ended questions) were coded and analyzed, and the findings constitute the bulk of the book.

The evidence indicated that different legislators held different conceptions of the various roles within the sectors, which the authors proceeded to differentiate with such terms as, for example, "trustee," "delegate," and "politico" (in the case of the core-roles sector's "representational" role), "broker," "inventor," "tribune," and "ritualist" (in the case of the core-roles sector's "purposive" role), and "facilitator," "resister," and "neutral" (in the clientele-roles sector's "pressure group" role). The percentages of legislators who held one or another of the various role conceptions in the different legislative houses of the four states varied, and the authors made some (occasionally strained) attempts to account for these variations on the basis of such factors as competitiveness of the legislative district and majority or minority party status within the legislatures. Finally, an attempt was also made to put together the typical "role combinations" or "role structures" in the eight legislative chambers and to correlate them with the predominant kind of cleavage in the state's politics.

It is difficult to evaluate *The Legislative System*. At times it reads like a major contribution to political science, and at no time can the reader fail to be impressed with the high quality, even brilliance, of the analysis, particularly that setting up the theoretical framework. The only question is whether the authors' intelligence, time, and energy could not have produced findings of greater substantive significance if a less rigid role-theory approach were used. That self-conceptions of the proper role of the legislator affect legislative behavior has been postulated in the past.[18] The truly significant questions for the understanding of the legislative process appear to be exactly in what respect that behavior is affected, or, if differences in role-conceptions exist, exactly what, if any, are the behavioral consequences of these differences. Yet the analysis stops at precisely this point: the different role-conceptions inferred from the answers given

[18] See, for example, Norman Meller, "Legislative Behavior Research," *Western Political Quarterly*, XIII (1960), 144.

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to the interviewers are described, but the reader never learns to what extent actual behavior conformed to the answers given [19] or whether different answers had different consequences on the legislative output—an omission that the authors implicitly attribute to "the usual limitations of time and resources, as well as the methodological difficulties of doing otherwise." [20] It should also be pointed out that although the theoretical framework recognizes the crucial nature of the relationship between the legislators, on the one hand, and the governor and administrative officials, on the other, exploration of this relationship was "omitted from the study primarily because none of the four authors' individual projects focuses on the administration." [21]

A few years ago V. O. Key casually referred to the concept of role as being a "bit recondite." [22] Whether or not this be so, the "role" certainly does not appear to be the most fruitful unit of analysis "in developing legislative theory 'properly.' " [23] At most, role conceptions are likely to provide only the limits within which discretionary behavior takes place rather than to constitute the crucial premises for decision-making by individual legislators. Indeed, some of the most interesting findings reported in the book were those that managed to sneak in while being, strictly speaking, independent of the analysis of role-conceptions: the political socialization, past career patterns, and future aspirations of the legislators; and the interest group constellations perceived by legislators in the different states and the perceived bases of influence of those groups.

V

The preceding discussion has probably suggested some of

19 This is truly regrettable. According to the interview data from both chairmen and members of committees, the chairmen were expected to be fair and not try to dominate committee decisions. If indeed chairmen lived up to these expectations, they would be acting differently from the way it is commonly believed their counterparts in Congress do—a finding of some significance. But it is important to know whether they did in fact so act.

20 Wahlke, *et al.*, 32.

21 Wahlke, *et al.*, 13, n.15.

22 See Key's *Politics, Parties, and Pressure Groups* (4th ed.; New York, 1958), 5.

23 Wahlke, *et al.*, 3.

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the strengths and weaknesses of the behavioral approach for studying politics. If Lane's *Political Ideology* appears to be the most generally satisfactory volume, this is in large part because it deals with a substantively significant problem that is particularly susceptible to the techniques of that approach. At least since Plato students of politics have known that the politically relevant beliefs, attitudes, and feelings—the political ideology—of the members of any political system have important consequences for the form and stability of its governmental structure. Reliable indicators of that ideology have until the past few decades been rare indeed. Lane's volume should make clear that when we want to find out what the common man is thinking about politics, we can do so best not by reading books about him by historians, philosophers, or even psychoanalysts, but by rounding up a representative sample of "common man" and proceeding to question him. In doing just this Lane once again demonstrated the great value of such behavioral techniques as sampling, the systematic interview, and content-analysis of interview material in the study of individual attitudes—an area in which various voting studies have produced impressive results in the past. Lane's interviews were more psychoanalytically-oriented than those of the survey researchers, suggesting that the approach pioneered in some thirty years ago by Harold Lasswell [24] and then long neglected may again be bearing fruit. The use of the systematic interview by the authors of *The Legislative System* also illustrates the practicability of finding out about the informal structure of a legislature or other governmental body by directly asking as many of its members as possible.

There can really be little doubt that the data-collecting techniques associated with the behavioral approach can often provide information not obtainable by other methods. The utility of the approach's stress on explicit conceptual or theoretical frameworks is more debatable. Of course, the use of some kind of conceptual scheme in research is inevitable: all research proceeds on the basis of various assumptions about classes of events to examine, the aspects of those events to note and record, and the most fruitful categories in which to organize the data. The actual choice concerns the degree

[24] Harold Lasswell, *Psychopathology and Politics* (Chicago, 1930).

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to which those assumptions will be allowed to remain unconscious, vague, and inarticulated or be set forth in an integrated body of propositions and hypotheses. Although few behaviorists are likely to deny that some outstanding contributors to the discipline have used no, or at least very primitive, explicit conceptual schemes (suggesting that some people's unconscious assumptions may be better than other people's conscious ones), there do appear to be advantages in the average investigator's making the explicit theoretical effort both in improving the quality of the particular project and in facilitating the cumulation and consolidation within the discipline of disparate research findings.

Nevertheless, *The Legislative System* and *The American Polity* illustrate two serious dangers that must be guarded against in the use of explicit frameworks: first, that the construction and elaboration of the frameworks become an end in itself, thus ignoring or defeating the research-oriented purpose that this kind of theorizing is supposed to serve; and second, that the vogue for constructing theoretical frameworks out of concepts borrowed from the other social sciences leads to research findings in political science that are non-behavioral or non-governmental or both. As an example of the first danger, however intellectually impressive the overall theoretical framework of the legislative system in the Wahlke, Eulau, Ferguson, and Buchanan volume, one can question whether the elaboration of the authors' model of the individual legislator did not get out of hand when the different role-combinations that can be derived from the various sectors and roles that were distinguished total in the hundreds. Of course, future research may show that most or even all of the distinctions are in fact useful, but until such time skepticism is definitely in order. With respect to the second danger, it has already been pointed out that despite the behaviorally-oriented framework of *The Legislative System,* the particular use that the authors made of the concept of role as their primary unit of analysis led to the bulk of their findings being at least one remove from the actual legislative behavior that political scientists must explain. Similarly, only Mitchell's wholesale reliance on Talcott Parsons' very abstract sociological model of social systems seems to account for the fact that in over 350 pages of analysis,

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purportedly of the "American political system" or "American politics," data concerning the structure and operation of the actual institutions of government are hardly ever mentioned.

Political behaviorists must remember that if they wish also to remain political scientists their central focus of attention should be precisely the processes and institutions of government. Other patterns of non-governmental behavior in society are of interest only insofar as they affect or are affected by these institutions and processes. Political behaviorists should also recall that originally the merit and distinguishing characteristic claimed for the behavioral approach was not so much the novel data-collecting techniques or the emphasis on explicit theoretical frameworks, but the moving away from the arid formalism of the more traditional school, which was content to describe government and the political process largely in terms of constitutions and legal structure, and the insistence on learning to speak of the governmental process in terms, as Arthur F. Bentley put it over fifty years ago, of the "actually performed legislating-administering-adjudicating activities" of men. "Activity," according to Bentley, was the "raw material" that had to be explained in the study of politics—"first, last, and always activity, action, 'something doing.' " [25] Yet statements such as "the authoritative mobilization of resources," "the integration of the system," and the like, are so many conceptual removes from any "actually performed legislating-administrating-adjudicating activities" that it does not appear inappropriate for even a friendly critic to caution against the great stress on theoretical conceptualization turning the behavioral approach into a new formalism more arid and stultifying than the old—and even less productive.

DEMETRIOS CARALEY

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[25] Arthur F. Bentley, *The Process of Government* (Chicago, 1908), 180, 176.

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REVIEWS

The Fascists in Britain. By COLIN CROSS. New York, St. Martin's Press, 1963.—214 pp. $5.00.

Mr. Colin Cross has written a well informed and notably objective study of the various movements which may be grouped under the general title of Fascism. They arose out of the stresses and disappointments which ensued after the First World War, became of some importance owing to the dynamism of Sir Oswald Mosley, and faded out with the outbreak of the Second World War and its aftermath. Just as Italian Fascism arose from the failures of the orthodox democratic parties in Italy to present a coherent plan for meeting the problems of a new age and so gave to Mussolini the opportunity to seize power under the slogan of the need for an individual to provide leadership for a nation, and just as Hitler took advantage of the disillusion of the German people with the democracies of the West which failed to live up to the ideals of President Wilson, so in Britain Mosley tried to take advantage of the failure of Ramsay Macdonald and Snowden to translate into action the ideals of the Labour party.

Mr. Cross throws much new light on what one might call the pre-Mosley era. I must confess that although an active politician during the whole of the period, I had never heard of Miss Lintorn Orman or Mr. Arnold Leese before reading this book. I knew, of course, that there were obscure groups of Fascists. Every now and again a youth would appear in the police court charged with making a disturbance, generally to be treated with great leniency and even sympathy by Tory magistrates afraid of socialism, but these incidents appeared to us merely as youthful aberrations not to be taken seriously. I knew too of the prevalence of some anti-Semitism. Indeed as Mayor and later M.P. in Stepney I had had to combat these tendencies, but they had little support. I myself had had to take to court a Brigadier who after describing Jews as engaged in drug smuggling and the white slave traffic called me a Jew, but that was about the extent of my contact with the movement.

As a movement for substituting the leadership of the individual man for democracy, pre-Mosley Fascism lacked a man. I recall very well the impact of Mosley when I entered the House of Commons in 1922. Here was this handsome young aristocrat with a charming wife moving away from the Tory party and

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appearing as a possible leader of the workers. A wealthy young baronet was just the person to appeal to Mr. Macdonald, while his vigorous invective made him popular with the rank and file of the Labour party. It seems to be part of the order of things that leaders of gross reaction should appear at first as leaders of the left wing.

Mr. Cross traces with admirable understanding Mosley's progress in the Labour movement. As a junior minister in the second Labour government I shared in the disillusion of the party with the utter failure of our Prime Minister and our Chancellor of the Exchequer to take any effective action in the face of the unemployment crisis. Young Tories like Macmillan and Boothby shared our feelings. I recall very well the party meeting when Mosley put forward his views with much of which we were in agreement and then by a tactical error called for a vote. This was the parting of the ways. From now on Mosley's arrogance disgusted the rank and file. Able and sincere young men like Bevan and Strachey soon found their leader intolerable. Throughout his career Mosley failed to keep the allegiance of men of character and standing. He was not a man of original ideas. At the start he adopted some of the ideas of J. M. Keynes, but progressively drew on the conceptions and methods of his German and Italian prototypes. In the thirties he became more and more of a nuisance. For some time some Tories regarded him as a useful ally against Labour. It was some time before the danger of his anti-Semitic campaign was recognised. It took much persuasion for the effective step of banning uniforms to be adopted by the Home Secretary.

I think that our author rather underrates the brutality of the methods employed by the Fascists though eventually the denunciation of the proceedings at the Olympia meeting by decent Tories like Terence O'Connor and Geoffrey Lloyd did much to discredit the movement.

A factor not mentioned by Mr. Cross is that of Cockney humour which was useful in debunking Mosley's theatrical displays. There was a good example at a meeting where Mosley under a spotlight strode on the stage with arm uplifted. Whereupon a voice said, "Yes, Oswald dear, you may go to the lavatory." Another was during a blitz. I came down to East London where a whole street had been destroyed. A little man came out of the ruins of his house saying, "Hullo, Major. Do you know the bloody fun of it? The first bomb dropped slap on Oswald Mosley's headquarters. Laugh, not 'alf."

Economic conditions in East London made it easy to rouse some anti-Jewish feeling, but it is to the credit of the East Lon-

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doner that the Fascists failed in their attacks on Labour strongholds. The Second World War put paid to the Fascist movement. Probably nowhere but in Britain would its adherents have got off so lightly. The vigorous policy of the Labour government after the war so greatly in contrast to 1919 destroyed the basis for a renewed Fascist movement and though Oswald Mosley carries on, as a discredited old man, there is little danger of any recrudescence of activity. The British people are too democratic and too experienced to fall for a rabble-rouser like Mosley.

Mr. Cross has done a good service in telling the story of the Fascists in Britain. It shows how even a considerable orator with lots of money and the backing of a Press lord failed to conquer British common sense and common decency.

EARL ATTLEE

HOUSE OF LORDS

The Deadlock of Democracy: Four-Party Politics in America. By JAMES MACGREGOR BURNS. Edgewood Cliffs, N. J., Prentice-Hall, 1963.—xi, 388 pp. $5.95.

According to Professor Burns, American government and politics are in a bad way. They are in a bad way because while "our economy and our national life have become integrated . . . our politics have remained fragmented." Institutional fragmentation through the separation of powers and the federal system is aggravated by a faction-ridden party system unable to govern either itself or the country. Except in a purely formal sense and then only as an electoral device once in four years, our boasted two-party system even at the national level is in fact a four-party system. Within both the Republican and the Democratic parties there is a Presidential and a Congressional party at loggerheads with one another.

The Presidential party, reflecting the historic posture of what Burns calls the Jeffersonian system, seeks to organize power "centrally and hierarchically through a national party led by the chief executive." The Congressional party, following a Madisonian tradition, rests on a system of "countervailing public offices . . . [and] competing political organizations," based not on national interests but on the parochial interests of the several states and Congressional districts. The Presidential party looks to numerical majorities composed of urbanized-suburbanized voters held together not only by the centripetal force of Presidential leadership but by having to fight competitive campaigns

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in doubtful states and districts at every biennial and quadrennial election. The Congressional party on the other hand reflects what Calhoun called concurrent majorities of sectional, state, and local interests with their base in non-competitive "safe" one-party states and districts. Its method of governance is not by numerical majority, upon which the Presidential party relies, but by "coalition, compromise, and consensus."

To compound this confusion the rules of the game as they operate in the organization and procedure of Congress have created a minority veto system as a substitute for majority rule. Leadership is a function not of ability or intelligence or even party loyalty but of political longevity. Survival at the polls, biennium after biennium, gives one a claim on committee assignments, chairmanships, and other party perquisites that outweigh all other considerations. And since the political survival rate of congressmen who come from "safe" states and districts is higher than that of their colleagues from doubtful districts, it is they who become the power elite in the Congressional party. If the policy preferences of these members of the Congressional power elite corresponded with those of the majority party either in Congress or in the country, this situation would create no serious problem. Unfortunately, as often as not, they represent the policy preferences of a minority, differing not only from those of the President but from a majority of their own party colleagues in Congress.

From their strategic positions as members and chairmen of important committees, including the powerful Rules Committee in the House, members of the Congressional power elite control the policy machine. Unable to enact its own program, whatever that may be, the Congressional power elite is nevertheless able to frustrate and defeat the legislative program of the President and the majority for whom he speaks. The result is what Professor Burns has called "The Deadlock of Democracy." This minority veto system is fortified by rules of procedure and organization, of which the seniority principle is but one. Others include the power of committees and committee chairmen to rewrite or smother legislation before it even reaches the floor, the all but unlimited power of the Rules Committee to substitute its will for that of the House, and the filibuster in the Senate. Part of the system also is the Electoral College under which a President can be elected with a minority of the popular vote and even, as in 1825 and 1877, with a minority of the electoral vote itself. The possibility of this happening again in 1964 is by no means chimerical.

The best that Professor Burns can say for this Madisonian

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model is that it "has provided flexibility, accessibility, and representativeness." But it has done so at the expense of "leadership, vigor, speed, and effective comprehensive national action." In more tranquil and less swiftly moving times, we could afford this archaic process. "Americans," says Professor Burns,

> have had the cushion of time. We have never been so close to the abyss . . . (Except when we went over it in 1861) that we have felt much urgency about strengthening our political system. If government could not cope with economic crises . . . if it was a generation or two behind other modern nations in welfare programs, if it could not meet its international obligations, if Negroes were still segregated . . . (and disfranchised), we could wait until a large enough popular consensus could be achieved to allow our divided government to act. There is grave doubt now that history still allows us this cushion.

What to do? One great merit of his book is that Professor Burns is quite as clear in his prescription as in his diagnosis, although the road to reform looks pretty rough to me. Basically what is needed is a reinvigoration and a reorganization of our major parties to bring the Congressional and Presidential factions into closer alignment. A sixfold path to this end is outlined and includes: (1) Control of national elections by the national government; (2) Party organization at the grass roots with "more emphasis on party clubs and less on party committees"; (3) The merging of Presidential and Congressional parties with a separate ballot for Presidential, Congressional, and Senatorial candidates in order to facilitate straight party voting for national offices; (4) Radical changes in Congressional rules to make possible new party leadership or rather to restore to the majority party leaders and their whips control of policy committees including the Rules Committee. Seniority and the filibuster presumably are to be mended or ended; (5) A revolution in party financing to extend the base and give the central party organization greater resources with which to finance Congressional campaigns and make congressmen more responsible to central leadership; (6) A revised, reorganized, and invigorated opposition by a similar close alignment of the Congressional and Presidential factions of the minority party.

Beyond these reforms, most of which are presently within the powers of the parties once the will to reform is aroused, there are others that will require constitutional amendments. These include: (1) a four year term for representatives to correspond with the term of the President; (2) repeal of the 22nd Amendment which undercuts Presidential leadership in the second term; (3) a change in the Electoral College under which "a Presi-

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dential candidate with a plurality in a state would receive two electoral votes (for the state's two Senators). The remaining 435 electoral votes would then be divided . . . on a nation-wide basis in proportion to the national vote for each candidate."

Professor Burns is the most persuasive of a considerable company intent upon making our Madisonian political system more effectively responsive to the needs of a nuclear age. Underlying most of these analyses from Woodrow Wilson to James Burns is a not always inarticulate preference for the more integrated, flexible, and presumably effective parliamentary system of which the British government is the prototype and model. But as we learn more about how this model actually functions, and as we compare the output of the two systems in this century, we may well question whether our own evolving Madisonian model is not to be preferred. Professor Burns wisely warns us against excessive reliance upon mechanical gimmicks in political science. Without great leadership, no political system is likely to lead a nation to greatness. With great leadership, almost any system will do. "The cure for democracy," says Professor Burns, "is leadership—responsible, committed, effective and exuberant leadership." Who can say that under Washington, Jefferson, Jackson, Polk, Lincoln, Wilson, and the two Roosevelts the American system did not compare favorably with any other then extant? No mechanical change could make a Lincoln out of a Harding, or transform Calvin Coolidge into a Theodore Roosevelt. Nor has the British model been proof against blindness, bungling, and impotence under its Baldwins, Chamberlains, and Edens. Not every Prime Minister is a Winston Churchill.

"The Constitution," said Senator John F. Kennedy, "is a very wise document. It permits the President to assume just about as much power as he is capable of handling. If he fails, it is his fault, not the system's." Did President Kennedy, before his death, agree with Senator Kennedy? If so, to whom or to what was the agony of his own frustration to be attributed?

PETER H. ODEGARD

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Manifest Destiny and Mission in American History: A Reinterpretation. By FREDERICK MERK with the collaboration of Lois Bannister Merk. New York, Alfred A. Knopf, 1963.—x, 266, xii pp. $5.95.

Long recognized as the leading authority on the acquisition of Oregon, Professor Merk here turns to the closely related topics of the annexation of Texas and the acquisition of California and

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New Mexico through a war of conquest. His chief concern in this volume is not diplomacy, of which he disposes with admirable conciseness, but with the appearance and significance of the notion of Manifest Destiny in its relation to the actual and proposed acquisitions of Mexican territory. The real heart of the book is the detailed description and analysis of the propaganda for the annexation of all Mexico, which flourished especially in 1847–48 during the last half of the war. Having traced the rise and fall of this grandiose vision, the author glances briefly at the use of Manifest Destiny as a rationalization of covetousness toward Cuba and Central America in the eighteen-fifties and moves on to a chapter on its revival, along with a new variety of racism, as a contributing factor in the insular imperialism of the eighteen-nineties. In a final chapter entitled "Mission," Mr. Merk concludes that a sense of Mission has been "a truer expression of the national spirit" than Manifest Destiny. Manifest Destiny, however idealistic in theory, in practice always cloaked acquisitive tendencies. Its "continental and imperialistic doctrines were never true expressions of the national spirit." It died with the nineteenth century. The idea of Mission, on the other hand—"dedication to the enduring values of American civilization," setting an example to the world rather than "overspreading" it, defending democracy in Europe when necessary—"remained alive, and is as much alive at present as it ever was" (p. 266).

Though Mission gets equal prominence with Manifest Destiny in the book's title and is briefly alluded to here and there, it is never really developed and seems brought in at the end almost as an afterthought. Manifest Destiny is the real subject of the monograph and is treated with the thoroughness of research and the philosophical approach characteristic of the author. Particularly valuable is the analysis of newspaper editorials in the years 1847–48, when the Democratic "penny press" of the Northeast, notably the New York *Sun* of Moses Y. Beach and the *Herald* of James Gordon Bennett, led the campaign of propaganada for the absorption of all Mexico. They were followed with slightly less assurance by their counterparts of the Northwest, but the Southern Democratic press was skeptical, while the Whig papers of all sections opposed the program. Opinion in Congress divided generally along similar sectional and party lines. Promptly upon receipt of Nicholas Trist's treaty *projet* from Mexico City, the all-Mexico program collapsed. From its quick and almost painless demise Mr. Merk concludes that the editors and politicians who had so loudly acclaimed it were probably speaking for themselves rather than for any very large public. Even the advocates of the program had been bothered by the problems that is raised,

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notably the problems of racial compatibility and the extension of slavery. Peace on the basis of the acquisition of only thinly populated areas of Mexico was greeted with a general sense of relief.

The ideal of Manifest Destiny in its purest form was "a free, confederated, self-governed republic on a continental scale" (p. 29). John L. O'Sullivan, coiner of the phrase and chief expounder of the idea, was, in Mr. Merk's opinion, consistently opposed to forcible annexation of territory. The components of the enlarged republic must come in voluntarily as Texas had done. This is no doubt true in the main, though O'Sullivan's comparison of the prospective "overspreading" of all Latin America by the United States with similar French and British prospects in Africa and India suggests a somewhat more imperialistic attitude. By others the phrase was certainly used to cover aggressive designs.

In his chapter on the eighteen-nineties the author minimizes, possibly too much, the influence of such racialists as John Fiske, Josiah Strong, and John W. Burgess. His case is stronger against Fiske and Strong than against Burgess; for although Burgess notoriously deplored the nation's venture in imperialism in 1898–1900, there remains the fact that in his massive treatise on political science and in his lectures at Columbia he had preached the duty of the "Teutonic nations" to govern and civilize the backward areas: "they must," he had written, "have a colonial policy." Why he should have been shocked when some of his distinguished pupils (like Theodore Roosevelt) undertook to practice what he had preached remains a mystery. Mr. Merk makes no reference to this contradiction.

JULIUS W. PRATT

WILLIAMSVILLE, N. Y.

Reinterpretation of the Formation of the American Constitution. By ROBERT ELDON BROWN. Boston, Boston University Press, 1963.—63 pp. $3.50.

Connecticut in Transition: 1775–1818. By RICHARD J. PURCELL. Foreword by S. Hugh Brockunier. Middletown, Conn., Wesleyan University Press, new ed., 1963.—xvii, 305 pp. $10.00.

The controversy over Charles Beard's interpretation of the United States Constitution has become so intricate and intense that Robert Brown's lucid summary of the anti-Beardian critique will be welcome to many readers. This compact volume, first

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presented in the form of lectures at Boston University, offers essentially the same argument as Brown's previous books on Massachusetts and on Beard, but bolsters the argument with material drawn from soon-to-be-published research on Virginia.

According to the "Brown thesis," America even before 1776 was a fundamentally middle-class society of independent farmers and comfortable artisans. Property restrictions on voting, consequently, excluded few adult white males; and it follows, Brown argues, that the American Revolution was a defense of democratic colonial legislatures against British imperialist aggression, not a struggle for more democracy at home. The men who drafted the United States Constitution were elected by these same democratic legislatures, and in all their deliberations took it for granted that government in America was already basically democratic, and would remain so.

The late Richard Purcell's account of post-Revolutionary Connecticut, in contrast, maintains that Connecticut was an "aristocratic, paternalistic" state until it adopted a new constitution in 1818. First published in a limited edition of five hundred copies at the end of World War I, *Connecticut in Transition* well deserved republication, for it lays bare a critical weakness in the current revisionist approach to the Revolutionary era.

Purcell does not disprove Brown's conclusions as to voting and the distribution of wealth, but he considers an additional dimension: power. In a society characterized by widespread affluence and an extensive suffrage, it may still be the case that decision-making is concentrated in the hands of an administrative elite recruited from a privileged class. Purcell thought this *was* the case in Connecticut. The same men were prominent in banks and insurance companies, on the Board of Governers of Yale, and as members of the upper house of the state legislature, the Council. The twelve "assistants" who, together with the governor and lieutenant governor, composed the Council, were elected by the state at large, met in secret session, and could veto any law or appointment: "in practice, this small body governed the state." Nor is it necessary to resort to the concept of "social deference" to explain why these councilmen were repeatedly reelected. Justices of the peace, appointed by the legislature and so "invariably Federalist," presided over election meetings in which (after 1801) nominations were oral and an open standing vote was required; moreover, the incumbent assistants were always voted on before other nominees. Purcell concludes that "the scheme of electing the Council was shrewdly arranged to satisfy the democratic demands, yet to leave it under control of the aristocratic governing class."

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The particular value of *Connecticut in Transition* is this attention to what Purcell calls "the working government," and what Timothy Pickering, in a letter to George Cabot in 1804, called Connecticut's "unwritten constitution." Professor Brown cites John Adams at least fifteen times, but he neglects Adams' description of the actual exercise of power in Connecticut: "The state of Connecticut has always been governed by an aristocracy, more decisively than the empire of Great Britain is. Half a dozen, or, at most a dozen families, have controlled that country when a colony, as well as since it has been a state." Samuel Brockunier, who quotes this statement in his foreword to *Connecticut in Transition,* points out also that while Brown thinks New England democratic even before the Revolution, Henry Adams called Massachusetts society in 1800 an "oligarchy."

Whatever historians eventually decide as to whether New England at this time was "democratic," it is doubtful that they will accept Professor Brown's application of that adjective to Virginia, a society in which (in 1790) 293,000 of the 748,000 inhabitants were slaves. Brown is also insensitive in his treatment of the compromises over slavery in the United States Constitution. The debates of the Constitutional Convention support Professor Brown's contention that "the really fundamental conflict in American society [was] the division between slave and free states, between North and South." But Brown reasons strangely when he goes on from this important correction of "the particular kind of economic interpretation" Beard suggested to the assertion that the Founding Fathers, in contriving a Constitution which made room for the peculiar institutions of both North and South, were sacrificing interest to principle. They themselves were the first to insist that they wished to harmonize existing economic interests, not do away with them; and I would have thought that by consenting to consider Negroes as property in a nation dedicated to the proposition that all men were created equal, the Fathers provided one of the more spectacular examples in history of the sacrifice of principle to expediency and profit.

STAUGHTON LYND

SPELMAN COLLEGE

The Urbanization of America, 1860–1915. By BLAKE MCKELVEY. New Brunswick, N. J., Rutgers University Press, 1963.—ix, 370 pp. $10.00.

This book is a substantial chronicle of the triumph of technology in the American city between the Civil War and the

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First World War. It records the impact on the city of all kinds of industrial developments—from the extension of the railroad across the continent in the eighteen-sixties to the appearance of the overhead trolley in a Ford factory around 1913. Mr. McKelvey also charts the changes in demographic distribution between 1860 and 1915 and gives an account of many political, economic, religious, educational, and cultural responses to these changes. In particular, he shows how New York City was forced to relinquish its claim to being the undisputed metropolis of the United States, and how within the framework of American democracy the small, relatively isolated, mainly commercial cities were supplanted by huge, interconnected, industrial-commercial, metropolitian regional complexes.

Using a wealth of material about some 140 cities, the author emphasizes the power of technology and commerce to lure people to urban centers. In 1860 Boston and New York already had congested slums and by 1880 population had spilled into suburbs. After 1880 the new residents of the West lived mainly in cities, and even in the South Negroes left rural areas in increasing numbers. It is not surprising, therefore, that "the total population growth west of the Alleghenies during any fifty-year period of the westward movement, including the cities newly built there, never quite matched the national urban increase of 35,000,000 between 1860 and 1910" (p. 61).

Mr. McKelvey does not distinguish explicitly, as did some of the great social thinkers of the period he studies, between a vast agglomeration of people linked by the cash nexus, and a humanly integrated community that can truly be called a society. Nevertheless his story of the efforts of municipal, educational, and religious reformers makes one realize—whatever his own belief on this matter—that community adjustments kept failing all along the line to keep pace with technological change and urban expansion. When the author finally characterizes the cities of America as "creative laboratories" just before the First World War, one is left in doubt as to how he evaluates them. We know that he believes they provided much greater material comfort, more intricate social organizations, increased technical efficiency, and even in places a milieu for creative artists. But because of a lack of interest in assessing the human value of these social forms, Mr. McKelvey almost completely avoids committing himself about whether the net moral effect of urbanization was positive or negative. Had he devoted more attention to the ideas of thinkers like William James, Charles Horton Cooley, Robert Park, Jane Addams, and John Dewey, he might have recorded more vividly the discontent that sensitive minds felt

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about the decline of community during this period of unprecedented industrial and urban growth.

Mr. McKelvey touches on the intellectuals' response to the city, but too summarily. He is adept at describing the nuts and bolts of urbanization, and at marshaling the facts about changing social institutions, but he is less than skillful in analyzing the ideas that developed in the American city. It is, for example, difficult to understand what he has in mind when he says (p. 281) that

> pragmatism, which subjected all theory to the acid test of practice, prompted a quick discard of many hopeful hypotheses and justified such a broad indictment of contemporary social, economic, and political systems, that even a courageous scholar like Richard T. Ely began to stress the primary need to accumulate more knowledge before taking precipitate action.

What hopeful hypotheses were discarded at the prompting of pragmatism? And how did pragmatism, as McKelvey suggests it did, encourage precipitate action with insufficient knowledge? One suspects that the author is not prepared to answer these questions and that his acquaintance with pragmatism is limited indeed.

If one thinks of urbanization more in terms of bigger than in terms of better populations, one can learn much from this book. But if one is interested in what Lewis Mumford calls the culture of cities—their mind and spirit—one will come away disappointed.

LUCIA WHITE

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The Powers of Government. By BERNARD SCHWARTZ. New York, Macmillan, 1963.—Volume I: *Federal and State Powers*—xiv, 470 pp. $12.50; Volume II: *The Powers of The President*—viii, 497 pp. $12.50.

In his preface the author characterizes this two-volume work as "the first part of a comprehensive commentary on the Constitution." This first part deals with the powers of government, leaving to a volume or volumes still unpublished the treatment of the vastly important subject of the rights of the individual.

As the writer of a systematic commentary the author has precedents, among others Joseph Story, *Commentaries on the Constitution of the United States* (1833); Charles K. Burdick, *The Law of the American Constitution* (1922); and John M. Mathews, *The American Constitutional System* (1932), all of which, of

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course, have become in many respects obsolete or at least inadequate. He has a meticulous guide or in some degree parallel performance in the several editions of the annotated Constitution as published under authorization of Congress, of which the latest edition (soon to be superseded) was edited by the late Professor Edward S. Corwin.

Somewhat like his predecessors but to a lesser degree, Professor Schwartz has followed the Constitution itself, proceeding less in terms of textual detail and giving more space to reasoned exposition based primarily on constitutional decisions down to the date of publication. The first of the two volumes is devoted to national-state relations, to Congress, and to the Judiciary; the second, apart from the nearly two hundred pages of footnotes for the two volumes, is devoted to the powers of the President.

While writing from the vantage point of a law school environment, the author assumes that constitutional law is much more than the private preserve of the legal profession and seeks to avoid the "arrant pedantry" of a legal treatise. He accepts, nevertheless, the handicap of the reality that his discussion "can hardly be presented with all the fluency of popular fiction." The treatment is elaborate and the discussion is rich, particularly, it seems, in such areas as that of Congressional investigations where the author has had first hand experience—as a highly controversial general counsel for an investigating subcommittee. The breadth of the treatment of some general topics and the strategy of avoiding meticulous proceeding, item by item, in the constitutional order add to the readability of the work, but may on occasion suggest a measure of arbitrariness. For example, in the treatment of broad categories of legislation there are quite properly chapters on taxation and commerce, but none deals with legislation in the field of the war powers. That important topic, or at any rate the exercise of war powers, is extensively discussed, but under another heading, that of the powers of the President. In this area classification in terms of conventional treatment of the separation of legislative and executive powers breaks down, giving way to such a differentiation as that between the taxing power on the one hand and the war powers on the other.

The work has the ring of authority—in this respect neither Justice Story nor Professor Corwin could have spoken with firmer tone—and seemingly the documentation and learning to support it. Without meticulous reading of a considerable portion of the book, there are difficulties in the way of appraisal. Such reading must proceed under handicap. Each segment of the discussion ought to be read in direct relation to materials cited. Yet

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the notes, instead of being conveniently placed at the foot of each page, are consolidated at the end of the second volume; an added inconvenience for the careful reader lies in the fact that whereas the running heads in the text are in terms of chapter titles, the running heads of the index are in terms of chapter numbers rather than titles, with the result that repeated reference becomes a cumbersome process. The reader therefore finds himself tending to accept the author's generalizations on faith as against making careful searches for footnotes. Examination made in the face of such difficulties reveals in general broad and thorough documentation. Yet it may also disclose the fact that a confidently stated generalization is supported only by a concurring or dissenting opinion of the Supreme Court, or even, as in one instance noted, merely by reference to an earlier book of the same author. The assurance and at times the eloquence with which generalizations are offered will carry conviction for the reader not too precise in his reference to sources and for readers who in any event seek primarily the point of view of the author in a realm of highly complicated materials. Highly critical readers, on the other hand, may be less sure of complete authoritativeness. In any event, the work is eminently worthwhile, and is likely to have extensive use over a considerable period of years. It remains to be seen how these volumes on the positive powers of government will look sometime hence when placed beside other volumes dealing with constitutional guarantees of liberty.

CARL BRENT SWISHER

THE JOHNS HOPKINS UNIVERSITY

Eichmann in Jerusalem: A Report on the Banality of Evil. By HANNAH ARENDT. New York, The Viking Press, 1963.—275 pp. $5.50.

Highly personal and impulsively written, this book is difficult to classify. Far more than a report on the trial of Eichmann, it probes deeply into the defendant's personality, retells in its essentials the full story of the murder of the European Jews, and contains a wealth of valuable and persuasive impressions, judgments, and vignettes. Not only does it do justice to Miss Arendt's reputation for brilliant insight and critical imagination, it reflects great courage, great independence of mind, and—other reviewers to the contrary notwithstanding—a shining humanity. These qualities of the volume overshadow by far its occasional unpleasant ones: sarcasm, some personal prejudice, an overdeveloped penchant for paradox, and a few lapses in taste.

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Miss Arendt is justly, albeit too scathingly, critical of the prosecution's (though not the judges') conduct of the trial. She attacks Ben-Gurion for having demanded a "show trial" which would serve a multiplicity of politico-educational purposes and scorns the chief prosecutor, Gideon Hausner, for having done the Prime Minister's bidding. The prosecution was largely successful, over some judicial objections, in putting on trial not only Eichmann's deeds but all aspects of the suffering and death of the Jews, and Miss Arendt is right in pointing out that the two stories cover only partially identical ground. But she formulates her objections in terms which indicate a surprising lack of empathy with the emotional turmoil the trial provoked in Israel.

Be that as it may, the presentation of a great deal of evidence irrelevant to the defendant's deeds greatly reinforced a tendency present in any case—that of overstating the case against him. And the unfortunate effect of this was to underrate the role of hundreds of other Eichmanns, many of them free and prospering in Germany today. Miss Arendt correctly implies this when she points out (p. 15) that "the logic of the Eichmann trial . . . would have demanded exposure of the complicity of all German offices and authorities in the final solution—of all civil servants in the state ministries, of the regular armed forces with their General Staff, of the judiciary, and of the business world." All these organizations, contrary to the widespread myth that the Jews were readied for the slaughter and ultimately despatched by a few thousand "SS gangsters," "were vying and fighting with each other for the honor of taking the lead" in this bloody business (p. 101). Whether or not the prosecution "carefully avoided touching upon this . . . almost ubiquitous complicity" (p. 15) because Ben-Gurion did not wish to offend the Adenauer regime, as Miss Arendt seems to have it, this fact did need recalling in the interest of the historical truth the trial was intended to present. Perhaps one should add, too, that, in these matters, the term "Nazi" has altogether been loaded with more freight than it will bear. Not nearly all the German participants in the murder of the Jews were Nazis. Or, if a more generous interpretation of the term makes it possible to describe them as such, the distinction between Nazis and Germans in the Hitler era does not hold nearly as much water as those who are enamored of it insist. Surely, they cannot have it both ways.

Miss Arendt persuasively sketches Eichmann not as a bloodthirsty fanatic, but as a mediocre, cliché-sputtering bureaucrat with very limited ideological commitments and interested only in advancing his career within the administrative and moral framework established by his superiors, the potentates of the

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SS/police system. He was but a cog, though an essential and crucial one, in the machinery of murder in which others had far more important positions. That the monstrous role which Eichmann played could be performed by such a normal type is far more horrible and a more devastating indictment of the Nazi system, of German society under Hitler, and of twentieth-century totalitarian bureaucratization generally than the picture of Eichmann presented by the prosecution. This is the "banality of evil" to which Miss Arendt refers in her subtitle. But far from denying Eichmann's own guilt she makes clear her approval of the fact that he was captured, tried, and hanged. To say that she sympathizes with and defends him—as one prominent reviewer has done—borders on the fantastic.

Whatever may explain and, indeed, have made them inevitable, two fundamental facts cannot be expunged from the history of the destruction of the European Jews: (1) Except for a relatively insignificant number of persons and instances the Jews went meekly to their death. Resistance was very much the exception—not the rule, as Professor Oscar Handlin has recently claimed in a remarkable effort at rewriting history (*Commentary*, November 1962, p. 399). (2) Far more horrible to contemplate, leaving aside the operations of the *Einsatzgruppen*, the actual administration of the "final solution" was in large part performed by *Jews*—the Nazi-appointed or approved communal leaders who compiled the deportation lists, the Jewish police who rounded up their fellow Jews for shipment, the "special commandos" which manned the gas chambers and crematories. In a very few passages, which have been used to distort and discredit the book as a whole, Miss Arendt restates these well-known facts (which none of her critics has even attempted to refute), ending up with the single judgment—unprovable in the nature of things, yet perfectly plausible—that if the Jews had been unorganized, if chaos had prevailed instead of the Germans' being able to rely on the efficiency of the "Jewish Councils," more Jews would have survived. These observations have prompted a series of violent, sometimes venomous, attacks on Miss Arendt. For doing very little more than reciting the *facts* (which has been all too rashly interpreted as "judging") she has been accused of "blaming not the murderers but the victims." Let it be said here that such imputations are beneath contempt. Not only is this an almost demagogic way of trying to inhibit discussion of the historical record, it is also completely wrong. As far as blame and guilt are concerned, nothing underlines more the diabolical nature of the Nazis than their success in the process of "administrative delegation"—their success in dehumanizing not only vast num-

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bers of Germans but also (though in an entirely different sense) many of their very victims. Far from exculpating the murderers, Miss Arendt, by pointing to the high degree of Jewish compliance and collaboration, adds to their indictment and highlights the unfathomable horror of their crimes.

Apart from some factual errors insufficiently important for a claim on the limited space available, Miss Arendt's distinguished work is marred by a few misstatements which cannot be passed over. To say that the Nazi regime became "openly totalitarian and openly criminal" only on September 1, 1939 (p. 63) will appear to the student of Germany in 1933–34 as doing violence to the facts. While we may attribute to her love of paradox her acceptance (p. 23) of Eichmann's claim that he was no anti-Semite, it is quite fantastic to state (p. 90) that his "conscience rebelled" at the idea of murdering German (as distinguished from non-German) Jews. To credit Eichmann with feeling "relieved" (p. 96) when the gassing program went into full operation because it granted Jews an easy "mercy death" of which earlier only the German insane had been deemed worthy—compared to the *Einsatzgruppen* practice of shooting the Jews—is nonsensical quite apart from the fact that the death meted out by the *Einsatzgruppen* was considerably easier than death in the extermination camps. To refer to Rabbi Leo Baeck (who was *not* "Chief Rabbi of Berlin," if only because there was no such office) as the "Jewish Fuehrer" in the eyes of the *Jews* (p. 105) is not only nonsense but in very bad taste. And it is hard to find a proper characterization for Miss Arendt's acceptance as proved of the more than dubious stories of Reinhard Heydrich's and Hans Frank's Jewish descent.

A good deal more serious than these errors is the fact that Miss Arendt has not seen fit to acknowledge how heavily she has drawn for her information, terminology, and even a few of her judgments on Professor Raul Hilberg's great work. In part this is due, no doubt, to her generally cavalier attitude toward citing sources. The *New Yorker,* in which the substance of her book first appeared in almost identical form, cannot, of course, carry footnotes, but this is hardly an excuse for the lack of such in the volume under review.

None of these defects, however, detracts substantially from the value of this honest, courageous, and, in many ways, powerful work. Far from being "soulless," it reflects not only its author's brilliant analytical gifts, but compassion, outrage, and a true sense of tragedy.

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Trade Union Growth, Structure and Policy. By H. A. TURNER. Toronto, University of Toronto Press, 1962.—412 pp. $7.50.

This important book contains a substantial core of comparative trade union history which, while of great intrinsic interest, is offered to the reader primarily as the basis of a stimulating challenge to certain time-honored propositions—including some set forth by the Webbs—in the area of trade union development and indeed as the basis of a critical analysis of unionism and industrial relations in contemporary Britain. Some of the major targets of Professor Turner's research into the "growth, structure, and policy" of the British cotton textile unions can, I believe, be summarized as follows:

(1) The skilled trades formed the historic "base" of English unionism because they alone possessed the ability to restrict entry into the labor market, an ability which was virtually essential to effective organization in a relatively pure laissez-faire society.

(2) Nevertheless, possession of the capacity to restrict entry did not establish the craft unions as direct descendants of the earlier medieval gilds, because the latter frequently protected the interests of entrepreneurs or of consumers and were, moreover, separated from the former by a period of time in which "continuous associations" did not exist.

(3) On the other hand, a direct relationship can be traced between the skilled crafts and later unions which included the unskilled, notwithstanding the inability of these occupationally heterogeneous groups to restrict entry autonomously and their (consequent) protection of a "standard rate" through collective bargaining. For while the skilled trades had been the first to organize, they influenced the governing institutions, administration, and policies which were adopted by the latecomers and which enabled them to form viable organizations (in contrast to earlier "movements" involving the unskilled).

In rebuttal to (1), Turner claims that skill has never been a necessary condition for restriction of entry—in fact, that skill could emerge as effect, rather than cause, of a well-policed system of apprenticeship which operated "by increasing the tasks that new entrants must master, and by specifying those tasks as monopolies of the properly-admitted" (p. 111). Thus the important distinction lies between the method of what one writer has termed "autonomous regulation"—and in particular restriction of supply by apprenticeship systems employed by "closed unions"

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—and the method of bargaining directly over the wage rate, as practiced by "open unions." Since the closed crafts and the medieval gilds both practiced apprenticeship, while the "New Unions" of the eighteen-nineties concentrated on collective bargaining, a case can be made for a strong family resemblance between the crafts and the gilds and against close kinship between the crafts and the open organizations. This assault on (2) and (3) is reinforced by another line of argument. Claiming that the early organizations in the cotton trades did qualify as trade unions because they manifested a strong "habit of association" and "a tradition of regulating working conditions derived from a previous age" (p. 106), Turner denies that "continuous association" implies "formal organization" maintained by permanent professional officers and regular sources of revenue. On the contrary, he maintains that the full-time officer was most often the product of a particular *type* of unionism, that is, the method of collective bargaining, which required professional expertise. (In addition, his valuable references to the importance of the employer status of various types of organized craftsmen inferentially blurs another line drawn between the craft gilds and the craft union.)

Having distinguished between the old crafts and the "New Unions" with respect to the method of advancement followed by each type of organization, Turner next denies the proposition that the former did exert a beneficial influence upon the latter. In some instances, he claims, the alleged relationship was reversed: on the one hand, some unskilled groups of textile workers (the Weavers) were organized before and even facilitated the subsequent organization of certain skilled trades; on the other hand, the existence of certain "closed" unions (the Spinners, for example) "cut off the infant general labour movement of the 1830's and '40's and postpone[d] its substantial reappearance for half a century" (p. 192). In fact, the latter had to wait for the emergence of a class-conscious leadership, impelled by political radicalism, and, indeed, for the security provided by legislative enactment during the First World War.

Nor, according to the author, can point (3) be supported by evidence gleaned from an examination of governing institutions and policies. For such examination yields more contrasts than similarities between the open and the closed unions. The open unions were often built from the top down and have tended to adopt centralized government with power gathered in the hands of dominant professional officers; the closed unions have retained a greater measure of local union autonomy and have vested more supervisory authority in their "lay" (or part-time) officers. (The

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closed unions are characterized by Turner as "exclusive democracies"; the open unions are "popular bossdoms.") And as a result of their retention of local autonomy, the closed unions have not been plagued by shop stewards' movements and are less susceptible to communist tactics at both the grass roots and the top levels. The open unions have tended to be very large, heterogeneous, and expansionist in order to maintain their standard wage rates; the closed unions have tended to be relatively small and to expand only to the extent necessary to retain control over entry to the occupations held by their "principal membership" or to counter employer organization. But the logic of autonomous regulation has obliged the closed unions to exercise sharp vigilance over the jobs in their respective domains; this has involved them in frequent "demarcation" (or work assignment) disputes; and it has made the closed shop "a necessary device of closed unionism." In contrast, the jurisdictional controversies involving open unions are likely to take the less troublesome form of disputes over members; and the union shop is all that the open unions aspire to by way of compulsory membership. And then Turner leads us to the general conclusion that

> . . . the least democratic unions, in terms of their membership's participation in union management or of its identification of the union with a shared and homogeneous interest, are the 'popular bossdoms.' But just because of their officials' relative detachment from their members, and of the diversity of interests which their leaderships must reconcile, these unions often tend to be the most progressive and experimental in trade questions, and to display a broader (or what it has recently become fashionable to call more 'responsible') outlook on the general policy of trade unionism (p. 305).

The foregoing was of course written before some of the large unions contributed to the frustration of a recent attempt to commit the TUC to the support of a policy of wage restraint, so that the author may have been somewhat optimistic about the potentialities of bossism. However, his query—"But in trade unions, are 'democracy' and 'responsibility' compatible?"—remains pertinent. Indeed it has been raised on this side of the Atlantic where predictions made at the time of passage of the Landrum-Griffin Act have since been unhappily verified.

Turner's insistence on the importance of occupational divisions of interest within the trade union movement strikes the American reader as highly significant, possibly because this phenomenon also has been so apparent in our country. However, one might accept this and other of the author's very interesting conclusions without subscribing to his prior contention that

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skill is not a very essential condition to restriction of entry—or, for that matter, that the crafts derived their economic strength solely or even primarily from their ability to restrict entry. One might ask why some trades were apparently able to restrict entry while others were not, or why various craft groups were unable to withstand the eroding influence of technological change. Professor Turner in effect attempts an answer to the first question by asserting that, after varying periods of organization within open unions, certain groups were in fact able to move toward autonomous regulation via "the appearance and elaboration of 'work rules' and 'job controls' which amount to informal attempts to control the supply of labour" (p. 254). Work-sharing and makework devices aim at the control of output or at imposing higher labor-capital ratios upon employers, but that is different from restricting entry, and indeed such rules can be enforced *autonomously* only if entry is restricted; otherwise they require implementation by the method of collective bargaining. Again, when the author asks why the less skilled weavers encouraged and accommodated themselves to the organization of more highly skilled groups, he finds that the greater irreplaceability (and versatility) of the latter during strikes accounted for a good part of the answer. Now this implies (a) that skill did indeed bestow anterior economic strength upon the possessor group and (b) that this strength lay not only in the ability to restrict entry to the trade via apprenticeship (which was never very great among U. S. crafts) but also in the capacity to bar entry to the work site during a strike—which of course constitutes a most potent determinant of the ability to deploy the method of collective bargaining.

LLOYD ULMAN

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Top Executive Pay Package. By LEONARD RANDOLPH BURGESS. New York, The Graduate School of Business, Columbia University and The Free Press of Glencoe, 1963.—xvii, 231 pp. $4.95.

In this interesting study Professor Burgess has addressed himself to the following problem. Since the last World War, coincident with the sharp rise in tax rates there has been, particularly for top executives, a rapid proliferation of compensation arrangements of a nonsalary kind with deferred and/or contingent features. And to many of these arrangements, either by law or because of the timing of receipts from them, differential tax treatment applies. There has been much conjecture and dis-

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cussion of what executives "really" get, and some attempts to measure the value of one or another of their compensation arrangements, with interest most recently directed to stock options. But in fact, the executive's rewards consist of a set of variegated items whose weight can meaningfully be assessed only by taking account of the whole package. And this is what Burgess has set out to do.

His study covers the three most highly compensated executives in the twenty-five largest manufacturing companies for 1929, 1937, 1942, 1952, and 1955–58. These executives comprise a group for which information is available from company proxy statements. On these data numerous operations were necessary.

For each of the major nonsalary forms of compensation—deferred bonus, retirement benefits (both pensions and other deferred-until-retirement compensation arrangements), and stock options—Burgess has computed a current pay equivalent (he calls it an income value) on both a before- and after-tax basis. The after-tax income value is the more carefully thought out and consistent measure. The meaning of the before-tax value is more equivocal.

No short discussion can do justice to his findings, but a brief summary will at least suggest the sort of thing he has done.

(1) "... the net movement before taxes of the average pay of the top executive was from $166,000 in 1929 to $268,000 in 1958, a rise of more than 60 per cent; the net movement after taxes was only from $140,000 to $142,000, or barely any increase at all; and the movement after taxes and on adjustment for cost of living was from $140,000 to $84,000, or a decrease of 40 per cent" (pp. 114–16). Parenthetically, it may be noted that, if the comparisons were made between 1929 and 1955 or 1956, the finding would be equality in cost-of-living adjusted after-tax rewards, the reason being the bunching of capital gains from the exercise of stock options in the two latter years (Burgess attributes all such gains, measured by the excess of market price over option price, to the year of exercise). However, these gains represent rewards for the effort exerted (or expected to be exerted) over a number of years, and an equally tenable view would be that the gains should be averaged over that span. Moreover, he does not consider current rewards to be associated with unexercised option even though they are heavy with gains. If changes in Burgess' procedures were made on both these scores, the conclusion could very well be that the "real" level of top executives' rewards in manufacturing was about the same in the later fifties as in the late twenties.

(2) But the weight of various components in this pay package

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was vastly different. Current payments (almost all of its salaries and fees) comprised 93 per cent of the after-tax package in 1929, but only 59 per cent in 1958 (by Burgess' measures considerably less than this in 1955 and 1956 when stock options were at a peak). Thus all other forms of compensation represented 7 per cent in 1929 and 41 per cent in 1958—this latter broken down into 18 per cent accounted for by flexible deferred compensation (stock option and purchase plans), 17 per cent due to post-retirement plans (pensions and long-term deferred compensation), and 6 per cent represented by short-term deferred arrangements.

Burgess of course goes into much greater detail than this for particular years, companies, and types of compensation plans. Space does not permit any attempt at a summary; we merely note that it is a richly embroidered and interesting story. Moreover, he pulls out findings not directly related to his main purpose but interesting in themselves and bearing on executive compensation. For example: age of retirement for the executives in his sample seems no different in the early part of his period (1929–43) when deferred compensation was relatively unimportant than it was in his more recent years when such pay loomed large in the total package (p. 71); again, there seems to be no difference in length of service in these two periods (p. 187).

As stated earlier, Burgess has made a real contribution. And it is important that this be stated. But it is also important to alert the reader that his findings are not definitive. Some scepticism is in order, and the "true" picture of executive compensation could be substantially different from what Burgess has sketched.

His findings are only as meaningful as the analytical framework and assumptions used in their derivation. And here, I think, there are a number of serious reservations. Some of them are matters of taste or judgment, that is, in my opinion, some other procedures are distinctly preferable to those chosen by Burgess, while some of them are difficulties recognized by Burgess but not remedied by him.

This is not the place to go into a mass of specific details, but it is of course necessary to explain the basis of my scepticism. I use deferred rewards as an example. In Burgess' derivation of the income value (salary equivalent) of a pension there are several points which are open to question.

(1) He makes an adjustment for mortality by assuming a life expectancy of fifteen years at age sixty-five, but he does not allow for mortality in estimating the income equivalent of the pension which is a stream of annual payments that must be made between the start of the pension plan (or its "phantom," about

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which more in a moment) and retirement. On this score, the income equivalent is understated in his study, for given some probability of death before sixty-five, a bigger stream of payments than Burgess derived would have to be made to build up a stake equal to the present value of the pension promise at retirement. His method assumes, in effect, that if an executive dies before retirement, payments toward his pension will continue up to retirement. And this of course is not the way such arrangements work.

(2) His pension valuation procedure completely neglects the death benefits built up out of the executives' own contributions under contributory plans. This is a valuable right over and above the pension that is promised, and hence neglecting death benefit is a real defect in the method.

(3) In computing the income value of a pension, Burgess spreads the stream of purchase payments required to support his pension promise over the executive's working life rather than the generally shorter period measured from the start of the plan; but it is a fact of life (consider the company's costs, for example) that this stream of payments starts only when the plan is introduced. Thus his procedure, designed as he explains to avoid a distortion, really avoids very salient interpersonal and intercompany differences. In practice, he considers the annual income values computed for these periods when the plan was not in effect to be "phantom" and properly does not include them in the executive's income if there is no plan actually in effect for him in a given year. But this means he is incorrect in his pension calculation for those executives for whom a plan is in effect, because that year's income equivalent is too small, being spread over too long a period, that is, one which includes the "phantom" payments as well. That is to say, the sum of income values actually attributed to pensions will always be too small to purchase that given pension at sixty-five even granting the suitability of all of Burgess' other assumptions, because the "phantom" part is never attributed; it is merely used to tilt the payments stream downward.

(4) Burgess recognized the problem posed by changing benefit formulae and the consequent necessity to supplement the initially computed annual income value by additions because of increased plan benefits, but he decided it would be too cumbersome to be useful (whether it would or not depends on how much the findings would differ, and this we are not really told). He therefore chooses to base his calculations on "the benefit that actually is received or that the executive expects to receive under the latest plan." This is wrong in principle and could be very

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wrong in fact, given the widespread liberalization of benefit formulae of the last decade. There is really no good reason for not using the correct procedure; it is cumbersome only for manual operations, not for an electronic computer.

These are some of the major differences on pensions, but they do not exhaust the list. Other reservations attach to his methods for getting the income values of other types of compensation. Put these points together and what emerges? Certainly not a proof that Dr. Burgess' findings are seriously in error, but rather a feeling that the "true" story on the income value of pensions and other components of executive compensation may not have been told. His figures may substantially encompass the story. On the other hand, different but at least equally defensible procedures may tell a noticeably different tale.

Finally, to revert to a more positive note, all this should not obscure the fact that Dr. Burgess has written an interesting and useful book which extends our knowledge and upon which others can build.

DANIEL M. HOLLAND

MASSACHUSETTS INSTITUTE OF TECHNOLOGY

Income Distribution and Social Change. By RICHARD M. TITMUSS. Toronto, University of Toronto Press, 1962.—240 pp. $4.25.

Wealth and Power in America. By GABRIEL KOLKO. New York, Frederick A. Praeger, 1962.—xii, 178 pp. $4.85.

These two books—one by an Englishman and the other by an American—are assaults on the proposition that income and wealth are more equally distributed now than in the recent past. Titmuss' approach involves a close examination of British income and wealth statistics and a description, sometimes amounting to little more than a list, of the devices available to the prosperous and the ingenious for the protection from taxation of their earnings and assets. Titmuss' major obstacles are to be found in the sketchy character of the British statistics and the almost deliberately baffling manner in which they are compiled and presented. British custom and British law make it exceedingly difficult to be certain even about such primary matters as the distribution of income by family or indeed the total incomes of given individuals from salaries, dividends, interest, and other sources. Thus a social analyst like Titmuss finds it almost impossible to adjust his basic data for such demographic variables

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as family size, female employment, and the income status of minors.

What Titmuss achieves within such severe limitations is a demonstration, first, that serious doubt attends all propositions about English income redistribution, and, second, that the wider use of capital gains devices, "top hat" pension and stock schemes, one-man companies, expense allowances, income covenants, and discretionary trusts, all serve to limit the impact of high income and inheritance tax rates. The major weakness of Titmuss' study at point after point is its author's inability to make even rough quantitative estimates of the importance of the devices he enumerates. A new Labor government could do few things more helpful to social change than to reform and expand British statistical services. As it is, Titmuss has cast doubt on the proofs of others, but has not proved very much of his own.

Kolko is in the infinitely more fortunate situation of using American data. Taking somewhat inconclusive issue with Kuznets, he argues that nothing fundamental to income distribution has altered in the twentieth century, that the Galbraith-Lipset-Riesman prospect of an emergent American middle-class society is mistaken, that poverty is a phenomenon scarcely less grave now than in 1900 even though its causes are somewhat different, and that the economy is dominated by a small group of powerful capitalists. On the last point, Kolko specificially denies the Berle-Drucker claim that the modern corporation and its masters have experienced important transformations of function and attitude in the last half century.

In this vigorously written short volume, Kolko has no doubt tried to prove too much. Even if relative income distribution has changed little, it is undeniable that rising productivity has improved the material position even of the bottom quarter of income recipients. Kolko seems appalled that in 1959 only twenty-three per cent of those who earned incomes below $1,000 owned their private automobiles. Surely there is another way of looking at such a fact. Even if Kolko's enthusiasm has led him astray here and there, he has reinforced the message of recent studies by Harrington, Miller, and Lampman which in various ways make it plain that income is not being more equally distributed, that poverty is a truly difficult problem, and that much of the apparent gains of the past were connected with high rates of employment which we have not enjoyed for nearly a decade. One can only wonder what Titmuss might have been able to prove if British statistics had not hobbled his efforts.

BARNARD COLLEGE ROBERT LEKACHMAN

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Federal Fiscal Policy in the Postwar Recessions. By WILFRED LEWIS, JR. Washington, The Brookings Institution, 1962. —xv, 311 pp. $6.75.

The influence upon business activity of changes in government receipts and expenditures is of two kinds, automatic and discretionary. In time of recession tax revenues tend to decline and expenditures to grow, reducing surpluses or increasing deficits of governmental units. Recently this tendency has been much enhanced in Western countries through the establishment of social insurance programs and pay-as-you-earn taxation. The resulting "automatic fiscal stabilizers" slow down and cushion declines in business, but they cannot of themselves convert decline to expansion. Hitherto recovery, if not left to "natural economic forces" (for example, the impossibility of indefinitely postponing capital maintenance and replacement), has relied on monetary policy. Yet a powerful instrument for managing the economy remains largely unused, discretionary fiscal policy. Economists are now pretty well agreed that a shift from budget surplus toward deficit, deliberately engineered through tax cuts or expenditure increases, represents a powerful means of stimulating economic activity; and that a tax structure that yields a deficit in recession may also yield a vast surplus at high employment, such is the sensitivity of tax revenues to business activity. Countercyclical variation of tax rates, through prompt legislation or through previously granted authority, now plays a vigorous role in Britain but has not yet been accepted in the United States.

Mr. Lewis, who at one time worked for the Bureau of the Budget, has used this valuable experience and inside knowledge in a masterly analysis of the working of both automatic and discretionary fiscal policy in response to the four recessions experienced by the United States since World War II. The contrast is striking. The automatic stabilizers have worked effectively within their limited sphere and are largely responsible for the mildness of the four declines in business. With respect to deliberate use of fiscal measures to combat recession, Mr. Lewis concludes:

> Progress can be discerned over the postwar period, from one recession to the next, in the public and political acceptance of unbalanced budgets as an appropriate means of combating recession. It remains true, however, that balanced budgets and "fiscal responsibility" are powerful ideas. Only for brief periods near the cyclical troughs has opposition been relaxed to deliberate additions to the passive budget deficits that inevitably accompany recession.

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Why is public and congressional opinion so backward? Mr. Lewis is not himself concerned with this question, but his book provides many of the materials for an answer. For example, the Treasury treats capital outlays, whether for public construction or for government lending, as current expense: what private business would be considered "fiscally irresponsible" merely because it borrowed to meet its capital needs? Again, the lag in collection of the corporate income tax accentuates the administrative deficit in the early stages of recovery, since tax receipts are reported on a collection rather than an accrual basis. Moreover the growing equities in the many trust funds administered by the Treasury never are offset against deficits in the administrative budget. Hence a substantial "deficit" may be reported by the Treasury at a time when accruing revenues much exceed current government expenses and when trust funds are accumulating assets at a rapid—from a stabilization standpoint, even a dangerous—rate.

Mr. Lewis discusses systematically the attitude of administration and congressional leaders toward fiscal proposals and offers many new calculations, especially of the surplus at high employment implicit in given fiscal programs. His book will be indispensable to students of the postwar economic history of the United States.

HAROLD BARGER

COLUMBIA UNIVERSITY

The Nature of Politics. By J. D. B. MILLER. London, Gerald Duckworth, 1962.—ix, 296 pp. 30s.

A disciplined and well stocked mind extending itself rather informally, less to persuade than to clarify, and as much to clarify for the author himself as for the reader, has here produced an unusually refreshing book. It is about the elements of politics and of political science; not written as a textbook, it will be a better one than many that are. It reduces political activities to an intelligible pattern, not a priori but from looking at and trying out arrangements of a great deal of comparative data. In so doing, it reduces political institutions as well to a novel and illuminating pattern. It *ends* with a chapter on the nature, purpose and limits of political science, by which time the reader is ready for the neat puncturing of some of the claims frequently made about what political science is or should be. Political science, it is argued, should not take as its purpose the "solution" of political problems, for, beyond a quite narrow range of prob-

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lems, mostly in public administration, the problems can only be solved by politicians in action. This follows from Professor Miller's view that "the real political problems are conflicts of interest; they represent, not forces blindly groping towards a single purpose, but a number of purposes in conflict with one another" (pp. 268–69). In such a situation, the political scientist may be used by one side or the other, but he will not solve anything. "If he has made 'solutions' the purpose of his study, that study may be nullified. It will, in any case, be handicapped because he will have accepted implicitly the argument of governments that they cannot provide money for political science unless it is useful to them" (p. 270).

For Professor Miller the essence of politics is disagreement or conflict between "interests" (which is broader than interest groups), each seeking to use "government" (that is, any kind of instituted authority, from a club executive to a national government) to resolve conflict in the direction it desires. And just as politics rests on disagreement, disagreement rests on social diversity (or awareness of it); and while some social diversities have been taken out of politics (for example, religious diversities in some countries), there are always new ones, so there will always be politics. Within this framework he proceeds to arrange and catalogue types of interest, of government, of ideas and opinions, of party systems, and of "recurrent political situations." His handling of the latter (Chap. XI) illustrates the ingenuity and clarifying quality of his approach. Since all political situations are matters of tension, he finds the regularities in recurrent situations by arranging them by types of tensions. Thus, in parliamentary systems, the tensions are between party and party, party and electorate, parliament and bureaucracy, minister and official, department and department, department and interests. There are different lists for autocratic and oligarchic systems, and a further list of tensions found in all systems. This classification enables him to set out a wide range of material in a revealing way; the material is all familiar but the overall picture is not.

Scholars may quarrel with some of Professor Miller's classifications or some of the assumptions underlying them. One might, for instance, question whether the degree of pluralism he attributes to all but primitive societies is typical of them all. Much of his analysis of politics depends on the assumption that societies (and individuals) are highly pluralistic universes. Thus he can assert that governments have as a general rule no single or overriding purpose such as is attributed to them by idealist and Marxist theorists, but normally have plural purposes. He rejects the Marxist bipolar view on the ground that it contains an

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indefensible assertion of moral superiority for one of the two overriding interests. But he does not consider whether, empirically, an overriding bipolar division of interests is not the general case, and an overriding multiplicity of interests the special case found only where there is both a democratic franchise and an expanding and prosperous economy, in other words, only in some advanced Western nations at some times within only the last hundred years. Had Professor Miller entertained this hypothesis he might also have questioned the adequacy of his sixfold classification of political systems in Chap. XII (established parliamentary, experimental parliamentary, militarist and random despotic, quasi-clerical, colonial, and communist) and have avoided the difficulty of having to fit Weimar Germany into the first category, which is the most highly pluralist. But to have entertained such a hypothesis would have been to weaken his pluralist description of politics, and thus to have deprived us of an excellent demonstration of the clarifying power and the limits of a thoroughgoing pluralist interpretation.

C. B. MACPHERSON

UNIVERSITY OF TORONTO

In Defense of Politics. By BERNARD CRICK. Chicago, The University of Chicago Press, 1962.—156 pp. $3.75.

This is, on the whole, an unpretentious, extremely civilized, and wise little volume which does not quite succeed in convincing one as to the correctness of its central premise. In developing his argument, however, Professor Crick makes a number of telling points which can very well stand on their own.

If I understand him correctly, the author wishes to establish both the autonomy of political theory, that is, its nondependence upon philosophy, and also the essential correctness of certain political values. These two goals are very closely related and both are approached in terms of a defense of "politics"; a defense of politics against ideology, technology, liberalism, conservatism, socialism, and democracy. The list of opponents may seem a bit unusual, but, in Crick's view, both liberal and democratic theory contain elements which are essentially anti-political.

As one might expect, the author's definition of politics is not a conventional one. Politics is not *merely* a struggle for power among groups whose aim is to control the state. A struggle for power is merely a struggle for power and nothing more. One can speak of politics, on the other hand, only when the "war of all against all" has been replaced by the agreement that conflicts among interests shall be decided by discussion, persuasion, and

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the mutual adjustment of goals. The willingness of groups to accept the political resolution of conflicts is the result of an awareness that in a divided society (and most modern societies are divided) the survival of the social order depends upon compromise. This awareness rather than any conversion to the virtues of freedom as an idea has been the major source of establishment of political regimes.

Thus regimes in which suffrage is quite limited may be political regimes, and those democrats and liberals who would replace the negotiations, compromises, and inefficiencies which are inevitable to a political situation by the rationality of administration or the purity of the general will are fundamentally anti-political or at least apolitical. Both of these alternatives assume that the natural multiplicity of interests can be reduced to unity, and, hence, share the basic premise of the totalitarian regime.

Now all of this seems eminently sound. After all, what civilized man would deny that discussion and accommodation are more intelligent forms of behavior than intransigence and violence? Who really would deny that all "legitimate" interests should participate in the process of reaching political decisions? Indeed, then, we may praise politics for itself, and we need not seek its justification in some underlying conception of the good which is brought to politics from ethics. In fact, the decision to establish a political regime fundamentally represents an agreement by civilized men to accept a situation in which varying conceptions of the good may freely compete (within certain fairly broad limits).

The argument, of course, is typically British and traces its antecedents to Hobbes and the foundations of liberalism. Unfortunately, as we have all known for some time, the moral autonomy of politics is not so easily established, and despite Crick's sophistication his argument falls heir to the same difficulties as those of its forbears. It simply is not true that a political regime emerges as the result of a Hobbesian compromise among interests. I need not belabor the point, for after stating the proposition Crick promptly drops it, although he never offers a substitute. However, the book itself is evidence of Crick's conviction that ideas do have some relevance.

I might add that Crick's own values are, as were those of his predecessors, more appealing than their theoretical foundations. For example, he strongly implies that freedom is a good thing even when no particular interest is at stake. Thus, I am sure that he would support the extension of religious freedom in Spain even though Protestants in that country do not constitute an important interest in any sense of the term.

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In fact a good many of the political values which we accept as individuals cannot be justified in purely pragmatic terms, and in many practical situations the possible consequences of our actions are sufficiently doubtful that we could not choose the more pragmatic course even if we wished to. Further, it is impossible to define the "legitimate" interests in any society solely in terms of existing groups, and every time one goes beyond such a definition one is relying upon assumptions which transcend the political context. In the last analysis, then, one cannot praise politics for itself. Crick draws upon values from outside his system almost every time he deals with a concrete issue.

One further point: I remain enough of an empiricist to insist that definitions should be developed as the result of empirical rather than ethical concerns, and I find little of empirical merit in Crick's definition of the political. Any limitation of the scope of politics which should exclude regimes such as that of the Soviet Union seems to me more likely to obscure than illuminate. I do agree, of course, that all political situations do contain an element of legitimate authority. War is not politics.

I fear that I have concentrated too much on criticism and too little on praise. That I have done so is, in this case, a sign of how close I feel to Crick's position, of how powerful a pull his argument exerts upon me, and of how cogently he has developed it.

STANLEY ROTHMAN

SMITH COLLEGE

The Governance of Mediaeval England from the Conquest to Magna Carta. By H. G. RICHARDSON and G. O. SAYLES. Edinburgh, Edinburgh University Press, 1963.—ix, 514 pp. $12.50.

For some thirty years H. G. Richardson and G. O. Sayles, independently and in collaboration, have been achieving high distinction by their publications in the field of English legal and constitutional history. They were bound finally to collaborate in writing a general constitutional history of England in the twelfth to fourteenth centuries. Here is the first volume. It is not surprising to find it an exciting book, reflecting the lively controversial spirit the authors have displayed in studies of Parliament and the *Modus tenendi.* In fact, it is by and large a critical commentary on the classic by Bishop Stubbs, *Constitutional History of England.* No doubt too much space, above all the first chapter on Stubbs' personal as well as scholarly weak-

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nesses, is devoted to criticisms many of which could appear more briefly in footnotes, with the text freed for a straightforward treatment of the royal government. Nonetheless the method stimulates interest; and, what is more important, it results in a constructive if not complete history of the "governance" of England from the Conquest to the reign of King John. The very disgressions into the personalities of Thomas Becket, Stephen Langton, and the barons who rebelled against John contribute to our appreciation of the problems of kingship and of church and state. (Possibly the authors are unnecessarily contemptuous of Becket, but it is refreshing to find a new justification of Henry II and the royalist bishops.)

What are the principal contributions? In the first place, the authors convincingly argue that the character of Anglo-Saxon kingship and such institutions as the jury (they reject the theory of a late Roman and Frankish origin) and the fyrd (which was a select army, not a "national" militia) remained important in the midst of a changing terminology and of developments under the Norman-Angevin kings. William the Conqueror was no great innovator. It was Henry I who created first in England, thereafter in Normandy, the most important governmental institutions of the monarchy of the twelfth century, namely, the office of chief justiciar, the Exchequer, and the justices-in-eyre. Henry II added details, increased the number of writs, and made the royal authority generally effective. As a result of the development of a centralized judicial and financial system (but the Exchequer by no means took care of all the royal revenues), the office of kingship was in part separated from the person of the king. In other words, here was an anticipation of the doctrine that the king is everywhere present in his courts. The monarchy was becoming public, rather than private and proprietary, with respect to conducting the business of the state.

In the second place, the authors appreciate the political and constitutional significance of the revival of learning, and particularly of the revival of jurisprudence through the study of Roman and Canon law in England as well as on the continent. Bishops educated to some degree in the two laws became royal counsellors, justices, and treasurers. Roman doctrines of public law supported the growth of the feudal realm and its gradual transformation into the state. (Yet the authors are not aware of the Roman origin of the essential terminology of the so-called inalienability clause which they believe was added to the coronation oath of Henry II in 1154. Although the terms were already in use, it remains doubtful that the clause was added before the late thirteenth century.)

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A few criticisms, besides those already given, are appropriate. On the problem of the public nature of kingship the authors should have made use of Ernst Kantorowicz, *The King's Two Bodies*. Indeed, there is little recognition of the importance of the political ideas of the twelfth century, for example those of John of Salisbury or of Richard Fitzneale, who in the famous *Dialogue of the Exchequer* speaks of the *status regis* as the public powers of the king based on the royal revenues. What do the authors mean by sovereignty when applying the word to the *regalis iustitia* (p. 255)? The need of the king to ask for the counsel of the great men or *proceres* of the realm was a principle that came from the Roman law and was known on the continent in the early Middle Ages. It was not, therefore, simply "in the English tradition" that "Glanville" stated the principle (p. 373). Why is there no reference to Jolliffe, *Angevin Kingship*, nor to Raymonde Foreville, *L'Église et la royauté en Angleterre*?

Altogether, despite the major criticism that too much space is devoted to attacking Stubbs, I have enjoyed reading this book. In a spirit of no-nonsense about traditional interpretations, basing their conclusions on a careful reading of the directly important sources, the authors increase one's respect for the English monarchy in the twelfth century.

GAINES POST

THE UNIVERSITY OF WISCONSIN

The Passing of the Whigs, 1832–1886. By DONALD SOUTHGATE. New York, St. Martin's Press, 1962.—xvi, 488 pp. $12.00.

If Whiggery is genealogy rather than ideology, the descendants of the Whigs must indeed "figure in our affairs" as Mr. Southgate puts it. But what, or who, passed in 1886? Holland's *Life of the Duke of Devonshire* is quoted to justify 1886 as a terminus because until then "the word 'Whig' was still in common use to denote a connection loosely bound together, the moderate Liberals, led by the chiefs of certain families of long standing."

But there continued to be moderate Liberals after 1886. Such chiefs as Granville, Spencer, Ripon, Rosebery, and Harcourt followed Gladstone, not Hartington, in 1886. We are left with the word Whig, and it was still in common use after 1886 to denote both Hartingtonians among the Liberal Unionists and moderate Gladstonian Liberals.

The title of this work leads one to expect a definition of

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Whiggery, or at any rate "a clear, short answer to the question, 'What happened to the Whigs after 1832?'" Neither expectation is fulfilled.

As for Whiggery, Southgate says (p. 77),

> To use the term 'Whig' simply to denote political opinions is to forfeit half the flavour and force of the word. A Whig who was part of Whiggery could hold any opinions he wished, however Radical, without forfeiting his right to the title, while a man who agreed in everything with Earl Grey or Lord John Russell might not on that account be regarded by the Whigs proper as one of themselves.

To the question how the Whigs proper were known, Southgate answers (p. 197), "A man was a Whig if Whiggery accepted him as such, and there is no more to be said."

Few will quarrel with the notion that "Whig" is more than a political label, but something more needs to be said if nineteenth-century Whiggery is to be distinguished at all from contemporaneous Conservatism. No doubt Whiggery "always bore the stamp of aristocracy," but from 1832 to 1886 more aristocrats, by any definition, were Tories, or Conservatives, than Whigs. Whatever the essence of Whiggery may be, it cannot be distinguished from Toryism by broad acres or ancient titles. Indeed, the cousinhood of Whigs and Tories appears as readily from Southgate's elaborate genealogical charts as do the interrelationships of Whig families, which are his point. Southgate simply fails to explain how the Whigs, thinking of themselves as a special breed of spaniels, could sustain their self-conscious atavism only so long as the conditions of party politics left the field to the family contentions of Whig and Tory.

Because the conditions of party politics changed radically from 1832 to 1886, an account of what happened to the Whigs in the period might be expected to explore, from the Whig viewpoint, the techniques of control in the small boroughs where, as Southgate shows, the Whig lordlings were most successful. Although he has read widely in the printed correspondence and biography of the period, Southgate has not used the manuscript sources most likely to have been of value here: for example, the Brand (Hampden) papers. Thus, there is no treatment of elections as such, of registers, expenses, or the nature of local Reform and Liberal associations. And there is no way to dovetail the effusions of Whig magnates, so copiously supplied here, with the hard realities of electoral business portrayed by Gash and Hanham. Southgate has not advanced upon the frontiers of our knowledge of party politics. There is no special virtue in "so

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often allowing nineteenth-century personalities to speak for themselves" unless some new questions are put to them.

The appendices and genealogical tables are difficult to use, though a monument to industry, owing to the quantity of curious symbols. For example, (p. 426) "The reference (Cl½, Cr, Bc) to the third Earl of St. Germans in Appendix I, Section III indicates that he can be found in the central section of Table B, on the right side of Table C, and also on the left inset of Table C." And while a Lewis Carroll logic may justify "%" to denote Peelites in Appendix II, a dagger marks them in the index, which is of persons only but unreliable. The work is profusely illustrated, but the author has confused Peel with the Prince Consort in his comment on the reproduction from *Punch* on page 261. Careless proofreading has left many textual errors.

BARRY MCGILL

OBERLIN COLLEGE

Bismarck and the Development of Germany: the Period of Unification, 1815–1871. By OTTO PFLANZE. Princeton, Princeton University Press, 1963.—vii, 510 pp. $10.00.

German *émigré* training and the critical involvement of the interested outsider have combined to produce some notable achievements in the American scholarship on German history prosecuted since the war, and on the basis of this first volume it can be confidently anticipated that Otto Pflanze's study of Bismarck will rank with the best of them. For here is historical biography in the approved grand manner, the public life of a major figure set solidly in the political relations and movements of the time, recounted with the judiciousness that is a blend of detachment and understanding, and presented with the articulated clarity and the modulations of gravity and whimsicality that are the marks of good writing.

Pflanze achieves his balance through a kind of division of labor in which he applies his critical acumen to Bismarck's ends and his empathic understanding to Bismarck's means. His initial section—the "years of preparation" from 1815 to 1858—is not so much biographical as analytical; it is a device through which he works up the main constituents of Bismarck's basic political disposition. Stroke by stroke, vitalized by a wealth of telling quotations, the features emerge of the conservative aristocrat, viscerally wedded to the Prussian monarchy whose weaknesses and needs he intelligently recognized and through a native volatility of temperament was prepared uninhibitedly to

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remedy. And to this portrait of a man accessible to a whole spectrum of traditional and revolutionary policies Pflanze adds, as an equally fundamental trait, the organizing principle inherent in Bismarck's rational acceptance of dominion by a reality in perpetual conflict and of politics consequently as the flexible art of the possible. Thus referred to the realm of means Pflanze, with Bismarck, enters upon the latter's subsequent career as ambassador, Prussian prime minister, and North German Chancellor and follows, in fascinating narrative detail, the explicit workings of the masterly technique of multiple choices with which Bismarck guided the Prussian monarchy through and around the shoals of particularism, liberalism, and international competition to hegemony in a united Germany.

The research is meticulous, the interpretations sound and consistent. Only two flaws worth the noting mar the work, and since these are of the more-or-less rather than the either-or variety they are of minor proportions. First, the organization of the work tends to segregate the analysis from the narrative. This is not, as I have indicated, a categorical division, since there are narrative portions in the former section and particular analytical pieces in the latter. The fact remains, however, that there is an imbalance in which the early analysis carries the burden of the later narrative, diminishing thereby some of its point and interest. Secondly, there is a tendency to depict German external relations—and particularly Prussian-French relations—primarily (certainly not exclusively) from German materials. Given the massive enterprise that has gone into the book, it is probably irresponsible to suggest additional burdens, especially since Pflanze's critical alertness rules out partiality, but ideally the pertinent sections would have been fuller if the French foils were given attention analogous to that vouchsafed the German liberal foils.

But over and above its large merits and negligible demerits the work as a whole raises a fundamental question. What constitutes originality in the writing of history? The question arises here because time and again in the reading this book vents an aura of freshness and novelty, and yet, when conceptualized, its framework and its results are undoubtedly familiar, notably to non- and post-German historians of Germany. To account for this duality and to make a final assessment we must recognize two kinds of historiographical originality. The first kind, which history shares with any intellectual endeavor, appears when a shift in the angle of vision shows every relevant fact and relationship, however familiar, in a new light and transforms the role of the subject as a whole in respect to other subjects. We

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may call this the extensive type of originality. The second, intensive type, more distinctively historical, accepts a known generic character and role for its subject but creates new knowledge by showing in an unprecedented way how its parts actually work together to make it what it is. Pflanze's is an original work in the latter sense. It is now the standard authority for Bismarck's early regime.

LEONARD KRIEGER

UNIVERSITY OF CHICAGO

In Search of France. By STANLEY HOFFMAN, *et al.* Cambridge, Mass., Harvard University Press, 1963.—xiii, 428 pp. $8.95.

It has happened before that France, fallen to a very low estate, has made a very quick and sharp recovery. But to assume that resurgence must inevitably occur *because* a point has been reached from which there is nowhere to go but up might be fallacious reasoning and inadequate explanation. It is the purpose of the collective search described in this book to measure and explain the nature of the contemporary phenomenon in France, from which analysis and appraisal of future prospects might emerge.

The decline of France has been of long duration, and for that very reason the writers of these essays all find it necessary to take a backward look into her history. Among them they make use of the approaches of the various social sciences, furnishing in the process proof of the limitations of these disciplines; the tentativeness of their conclusions they do not seek to conceal and one may even say that, to one totally unfamiliar with the French complex, a somewhat bewildering picture would be conveyed. But, granted some acquaintance with it, much that they write is enlightening, if also not altogether new; for they are all highly competent observers, possessed besides of the advantage of sympathetic approach that does not interfere with objectivity of judgment.

From their various analyses a consensus emerges. The recovery at the economic level and the transformations that have taken place in the structure of the economy of France are the clearest and the most marked aspects of renewal, much of which is, in this domain, concretely measurable. The reasons for so far-reaching a reversal are less clear, as are those for the change in the demographic situation. Kindelberger is critical of most of the usual explanations, and rather short on his own, but he quite rightly points to the importance of psychological factors. The

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success of the twentieth-century St. Simonians since the war has in considerable measure rid France of her economic inferiority complex; hence the enthusiasm of much of the business community for the Common Market, following its initial reluctance to plunge into uncertain waters.

The structure of French society has been conservative and rigid. Modification of it is a far tougher problem than change in economic practice, and the accomplishments so far have been mixed. Much of this book is properly sociology, a discipline to many of whose efforts the epithet "dismal" applies, but which can also, in proper hands, produce most enlightening results. Wylie's keen and sensitive analysis of the rural situation deserves very high rating, but Pitts' attempt at a similar task for the bourgeois and urban sector seems more open to question. Some perceptive observations are made on the concept of "prowess" and on the synthesis between the aesthetic-individualistic and the doctrinaire-hierarchical strands in the French culture, but these are also loosely defined concepts which can lead one astray. Hoffman, a political scientist, also has to resort to sociological analysis; the high quality of some of his observations could have been couched at times in simpler language.

It is in the political domain that change has been least successful; hence that is where the greatest tensions and the sharpest breaks are to be found. The Third Republic served a useful purpose, but the stresses of the twentieth century proved beyond its capacity for adaptation. The present French régime, the outcome of this state of affairs, leaves open the question of whether the Fifth Republic will serve the purpose of successful transition or ultimately constitute a setback. But in the foreign field the relative lack of interest of the bulk of the nation—in comparison with its concern with domestic affairs—has made adaptation easier. This is traced and explained by Duroselle with convincing clarity. His contribution is recommended reading to any who would obtain a balanced picture of what French policy has been and is, focusing on the deep-rooted and fundamental rather than on the transitory and sensational.

François Goguel sums up the whole in urbane fashion, pointing out, with perhaps excessive gentleness at times, the qualifications and limitations of the undertaking. The contributors all agree for that matter on the tentativeness of their interpretations, and just because France is undergoing so deep a transformation, much more will have to be written before she is—if ever—adequately understood and explained.

RENÉ ALBRECHT-CARRIÉ

BARNARD COLLEGE

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The Boxer Uprising: A Background Study. By VICTOR PURCELL. New York and Cambridge, Cambridge University Press, 1963.—xiv, 349 pp. $8.50.

This is more than an account of the year or two during which the Boxers and later the Ch'ing government were feeling their way toward a united opposition to the foreigner within their country, and to the final outraged thrust against the missionaries, their converts, the merchants, and the foreign legations in north China. In fairly leisurely fashion the author (lecturer in Far Eastern history in the University of Cambridge and for many years before that a resident in eastern Asia) describes the government, armed forces, and society of China before the days of active conflict with the West and with Japan, the shock of this impact culturally, politically, and economically, and the fumbling and frustrated attempts at reform. He studies, too, the problem of "anti-foreignism" in the nineteenth century: were the feelings of the Chinese and Manchus directed against business interests, against Christian missionaries as a group, or particularly against the Roman Catholic church, against the political and military actions of the foreign powers, or against all of these together? There follows a useful sketch of the history of various sects in China, their ideology, and their connections with a succession of rebellions, from B.C. 209 through the nineteenth century.

A group known as Boxers are accused in 1727 by the then Manchu emperor of stirring up the "stupid people" under pretext of practicing their boxing cult, and an order is issued for the suppression of their activities. The *society* of the Boxers (in Chinese, *I-ho-ch'üan*) first appears in history in the province of Honan in the rebellion of 1796—one of the early signs of the decay of Manchu power. It shows up again a few years later, also in Honan, northern Kiangsu, and Shantung, precisely the same regions in which it was to come to flaming life a century later; but for decades it was shrouded in obscurity. What was its *raison d'être*? Probably it was anti-dynastic at first, as well as anti-foreign, though the evidence is conflicting. Some time about October 1899, it became definitely pro-dynastic as well as anti-foreign ("Protect the Ch'ing; exterminate the foreigner" was the slogan) to the delight of the Manchu governor of Shantung, Yü-hsien, later to become the governor of Shansi, and the official responsible for the massacre of Protestant and Roman Catholic missionaries and their converts at T'ai-yüan and elsewhere. At heart the members of the society may have continued to be anti-dynastic, not trusting the ability of the officials to

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govern properly and resist the outsider. But besides being "anti," the society seems to have had no program of reform, no ideology to replace State Confucianism; its sole progressive principle was the equality it accorded to women.

Were the foreign envoys, stationed in Peking, as has so often been asserted, really sluggish in representations to their governments about the explosive situation that was developing? Purcell thinks not. But he makes a counter charge: "By calling up the legation guards from Tientsin on 30 May [1900] they precipitated a general rising which might otherwise not have taken place." This, added to the proposal to make a naval demonstration in north China waters (the people at the coast already being excited by the German occupation of part of the Shantung peninsula and the Italian demand for a slice of Chekiang), brought things to a head. Even as a small boy at the time, I can recall the Boxers at this moment swarming into Tungchou (twelve miles from Peking), where our family resided. My mother's notation in her diary for June 3 reads: "Yesterday there were four camps of Boxers three miles from here and less, and our people were sure that we were to be attacked last night." We could hear and see them, though they did not touch us then.

The author has covered his subject well. His sources include the most authoritative collections of Chinese documents—three all told, one published in 1951 and two in 1959—as well as English, American, Russian, and Japanese works. A special feature at the end is two appendices, one shedding new light on the problematic diary of the Manchu Ching-shan, the other giving excerpts of material from British missionary archives, hitherto almost untouched, although the appointees of the London Missionary Society and similar bodies were quite as exposed as those of the American Board of Commissioners for Foreign Missions (Boston).

L. CARRINGTON GOODRICH

COLUMBIA UNIVERSITY

The Politics of Scarcity: Public Pressure and Political Response in India. By MYRON WEINER. Chicago, The University of Chicago Press, 1962.—xix, 251 pp. $5.00.

The study of government and politics in India is a lively and fruitful field for research, especially for those concerned with theoretical and conceptual problems in the social sciences. Ideological disputation abounds—in Parliament, in the press, at public and private gatherings—and is not relegated to the academic journals or to a few stubborn essayists, as in much of the Western world. India's constitutional government, organized in a rather

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complex federal system, intertwines in its experimentation with national economic planning and attendant social change. The country is so large and varied and possesses so many colorful, articulate, and ambitious leaders in all branches of society that the challenge to the analyst is formidable. Furthermore, traditions of an ancient past persist, overlapping what has been modernized and forcing the serious student to master both classical and modern facets of the Indian heritage. It is likely that mature studies of Indian politics will lead us to alter some of our general theories in political science, so impressive has been the depth of experience in this country of nearly half a billion people.

Myron Weiner has been one of a very few political scientists who have recognized that to understand Indian politics one must immerse oneself in the subject matter. This empirical and behavioral approach is essential, especially because the "anti-politics" atmosphere endemic in New Delhi characterizes leaders of Indian public opinion and, unfortunately, the minds of India's political scientists as well.

A notion common in India is that if men were both good and rational, "correct" solutions would be found for each public problem as it arises, thus eliminating, in the national interest, the waste of lengthy and heated debates, concessions, patronage, and the rest. The dynamic of politics—the view that good and rational men may and often do disagree legitimately on given issues and that government is the mechanism for resolving these differences—is confronted daily by the urge for consensus in India. In fact, controversial "politics" is the normal course of Indian public life. Indian politicians have little to learn from abroad concerning the tricks of the trade for office-seeking or manipulating power for party or sectional ends. The trouble is that many Indian politicians and most alert citizens think that such behavior in practical politics is dirty and contemptible. Political scientists, agreeing with their lay contemporaries, escape responsibility for studying the facts of Indian politics by composing endless, boring tracts on the Indian Constitution or on similar formal institutions of the past.

The Politics of Scarcity is a study of pressure groups and their roles in shaping Indian political decisions. Professor Weiner conducted most of his research for this provocative book in West Bengal, although he has woven in relevant material from many other parts of India. In addition to a great deal of fresh information on pressure groups, he poses an important theoretical proposition: the advisability of developing in India a "politics of accommodation" that would recognize the multiple interests,

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regional variations, different rates of economic growth in various segments of the economy, differing rates of social change, and varying levels of competence that characterize Indian life, conceding a benefit here, denying a privilege there, toward the goal of creating a more vital, progressing society. What, in effect, Weiner suggests is that India accept "horse-trading" in politics as a natural and legitimate technique for growth in a country of plural interests. His suggestion is unpopular in India, however, since a "politics of accommodation" flies in the face of the consensus bias so deeply imbedded in the theories (if not the practices) of India's leading politicians.

It is not unusual that an American political scientist should present such a thesis. Although men in India's public life blanch at the thought, the United States, not Great Britain, has more in common with India as far as political circumstances are concerned: regionalism and sectionalism; cultural variation; a federal system of government; a political party system where power naturally tends to consolidate in the powerful states; dispersed industrial centers; urban power conflicting with entrenched rural interests; North-South and East-West axes of conflict—the parallels are many, despite the enormous differences culturally and historically. But Indian public leaders want a unitary "sense" of government, despite the federal system; they want disciplined national parties, not loose confederations of local interest parties bound together for purposes of general elections; they want centralized, directed purposefulness, not a "politics of accommodation." My prediction is that the facts will win out over wishful thinking, and that Weiner's book will appear to be more relevant ten years hence than it is today.

I have only one criticism to make of Weiner's excellent new book: the criticism really is directed at the Committee on Comparative Politics, with which Professor Weiner has been associated for some years.

In an effort to construct a reasonably sustainable general theory of comparative politics, the Committee has encouraged many studies of non-Western societies, posing the big issues that parallel insights and data now fairly well understood in the context of Western European and American experience. The questions posed are the big, sprawly ones that give hope of eliciting significant and large-scale answers. The difficulty is that there do not exist in most parts of Africa or Asia, certainly not in India, hefty collections of monographs, detailed articles, survey research data, or statistical compilations that can be drawn upon by the upper-level analyst to support his research directed at general theory.

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For India, this should be the decade for clean descriptive monographs on limited political themes; for single constituency studies in depth; for urban administrative histories; for village, town, district, and state political studies; for political party histories, preferably on a state or even more modest scale.

The case has been made that a few may be able to break through the "area studies" mold, cast off obsessions with language study, literature, history, and the arts, and—thus unimpeded—crash through with guide lines for the drones to fill out over the years. The theory is untenable and reflects a gross misunderstanding of the nature of the problems facing the analyst of non-Western societies. The first stage of research for the social scientist in an alien culture is to comprehend the style of that culture. For that, there is no easy or quick road.

Professor Weiner, like many of his colleagues on the Committee on Comparative Politics, avoids most of the absurdities of the rigid typologies erected by the Committee; he gives lip service (or at best modest attention) to the Committee's instructions, and then goes ahead to say what he has to say. The Committee is on the right track. For Asia and Africa, however, it is unwisely impatient.

RICHARD L. PARK

NEW DELHI, INDIA

Into Exile. By RONALD SEGAL. New York, London, and Toronto, McGraw-Hill, 1963.—319 pp. $5.95.

An African Explains Apartheid. By JORDAN K. NGUBANE. New York, Frederick A. Praeger, 1963.—xi, 243 pp. $5.50.

Cold Rebellion: The South's Oligarchy in Revolt. By LEWIS W. JONES. London, Macgibbon and Kee and New York, British Book Centre, 1962.—203 pp. $5.95.

These books deal with the problem of color. Two concern South Africa, one the United States.

Into Exile is an intensely personal document. The author was born into a well-to-do Cape Town Jewish family, but developed into a highly sensitive individualist unhappy with formal Judaism and revolted by a society which lived by the exploitation of the black majority while through apartheid it denied them, in his opinion, recognition as citizens or even as human beings. Segal enlisted in the battle against apartheid in many ways but chiefly by founding, editing, and personally subsidizing the now defunct magazine, *Africa South.* It contained

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contributions by distinguished liberals from South Africa and elsewhere, some of which could have been published in no other journal in South Africa. The book is a detailed account of his reactions to his life, of his various anti-apartheid efforts, of his activities and problems as editor of *Africa South,* and of his friendships with African leaders. Segal was denounced by the government as a "most dangerous agitator" and had to flee into exile.

Ngubane's book, in marked contrast to Segal's, is surprisingly objective, considering that the author is an African. For instance, he states with sympathy and understanding the white Afrikaner's oft-repeated cry that South Africa is their only home, that unlike the English they have nowhere else to go. He also says forthrightly that "the Afrikaner's fear of being swamped by the Africans in a non-racial society constitutes a challenge any statesmanlike approach to South Africa's race crisis must take into account" and he does just that in his own proposals.

The book opens with a fast-moving account of race relations in South Africa from the landing of the white man three centuries ago, the longest period of interracial contact in history. The author then spells out the influence of Western values, especially as evidenced by some early missionaries and some policies in the former British colonies, on the attitudes and cultural values of the African. This, he states, shows in the African's opposition to a racialist approach as represented by the African National Congress and such leaders as Chief Luthuli, Dr. Xurna, and Professor Matthews. His great concern is that the present policies of South Africa may result in strengthening Africans in and out of the republic who see no solution to present problems save a black South Africa. This more radical and racial approach which Ngubane so deplores he shows to be encouraged by the communists.

Several chapters give an exposé of the activities of the communist underground. For this the South African government should be grateful but it is not. Ngubane suggests this is because, while it easily could obliterate the weak and illegal communist movement, it does not since it uses the provisions of the Supression of Communism Act against those who oppose the official apartheid policy.

Not surprisingly, therefore, Mr. Ngubane has been "banned" by the South African government since the publication of his book under the terms of this very act, ardent foe of communism among his own people though he is. He is prohibited from attending *any* gathering, from entering *any* African area other than his own village, and from writing for journals or *any*

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publication. He must report weekly to the police. Since Mr. Ngubane is a journalist these bans make it impossible for him to earn a living.

This incident reinforces the reviewer's judgment that Ngubane's intelligent and moderate program for working out South Africa's racial problems has little hope of acceptance. Here, alas, Mr. Segal's pessimism appears more realistic.

Dr. Jones' book would earn him banning if the United States were following South Africa's way in race relations. It is, however, an indication of America's racial tensions that no American publisher would accept the book. This is a great pity, for there is valuable historical material in the volume as well as plenty of evidence of the more recent efforts of the South's oligarchy to nullify the laws of their nation and the decisions of its courts on matters of race relations.

The introduction demonstrates from actual voting data how small a proportion of possible voters exercise this function of citizenship in the South, a fact which greatly eases achievement of the oligarchy's purposes. Dr. Jones' opening chapters sketch the rapidly changing economy of the South and the inequalities in education, health, and other matters which the Negro suffered. Recorded also are the slow steps taken toward improvement in the direction of civil rights and in the armed services. Part I concludes by spelling out the gains that have been made since 1950, as well as the oligarchy's struggle for survival.

In Part II, while due credit is given to those Southern states which have embarked on ambitious programs to make facilities for the Negro more nearly equal to those of whites, Jones also shows the price all America is paying for disrespect of the law shown both by governors and the White Citizens Councils, and because of knowledge and skills withheld.

All this and more is familiar. The unique value of this book is that it reveals a Southern Negro, a highly competent sociologist, applying the techniques of his profession to an analysis of race relations in the South. Jones' sympathies are clear but his presentation is objective, loaded with facts and illustrated with well-chosen case vignettes. It is to be hoped that some American publisher will make this volume available here.

EDMUND DE S. BRUNNER

WILTON, CONNECTICUT

Mexico: Revolution to Evolution, 1940–1960. By HOWARD F. CLINE. London and New York, Oxford University Press

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for the Royal Institute of International Affairs, 1962.—xiv, 375 pp. $6.75.

Dr. Cline's book is the latest in the Royal Institute series of studies of individual Latin American countries. Like the others, it tries to deal not only with the country's politics, but with every phase of its geographic, economic, social, and demographic reality.

Howard Cline's talents make him especially well suited to undertake this type of enterprise; he is a historian notable for the range of his interests and for his willingness to make extensive use of statistical materials. These attributes are well demonstrated in the book under review, which deals with everything from irrigation works to Indian languages to recent novels, as also is Dr. Cline's familiarity with an impressive volume of literature.

One has regretfully to report, however, that in *Mexico: Revolution to Evolution* Dr. Cline's strengths become his weaknesses; instead of a systematic study dominated by a central organizing concept, one has rather a flood of unintegrated items of information. In the first place, there is simply too much material; seventy-six tables are interspersed among the pages of text, for example, with another nineteen in an appendix, not to mention twenty-five maps and five diagrams. More important, although the tables have been carefully assembled and sometimes reworked by the author, they are essentially undigested; they are rarely used to demonstrate the crucial points made in the text and few of them have data arranged in time series, although in most of the book the author is discussing trends.

The problems which this poses are aggravated by the organization of the book. For 329 pages of text, one has no fewer than thirty-four chapters, each dealing with topics so discrete that one must on occasion hunt through three or four widely scattered chapters to follow a major theme.

The lack of selectivity in the data assembled in tables extends also to the secondary materials used, the author tending diffidently to rely on quotations, from a varied assortment of sources, to support the points he makes.

Finally, one misses a satisfactory treatment, either in the text or in the tables, of the major questions which any evaluation of the distinctive road that Mexico has taken must answer. Some of these are discussed, but the issues are blurred instead of being focused clearly, and the statistical evidence is not brought to bear on their resolution; others are not discussed at all.

One such question which is not dealt with in the discussion of land reform is the effect that type of land ownership has on

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productivity, other things (like fertility and mechanization) being equal. This surely has relevance to any evaluation of land reform, and data are available.

An example of a central question of this type that is taken up but not well handled is, "How democratic is the one-party system?"—a perennial favorite, and, of course, central to any evaluation of Mexico's course. One might have expected that Dr. Cline, using his own good sense and his familiarity with Mexico, might tick off the evidence pro and con and arrive at a judgment, balancing, for example, the general freedom of speech and the press against occasional arbitrary acts like the jailing without trial of Demetrio Vallejo for strike activity and of David Alfaro Siqueiros for protesting Vallejo's arrest (this item is instead referred to much later, and then in a quite different context). Instead, in his zeal for a statistical answer, Dr. Cline simply cites Russell Fitzgibbon's surveys of rankings of countries by various putatively democratic characteristics which are interesting and suggestive but nonetheless impressionistic and based on several assumptions whose truth is by no means self-evident—despite the "specialized techniques and IBM computers" (pp. 172–173) which may be used—and leaves it at that.

Other failings of the book might be cited, among them, for example, the lack of treatment of the inflation and of fluctuations in export prices in the discussion of economic development. The book's strong point remains its comprehensiveness, its making available so much material in a limited compass. Nevertheless, the overall verdict must be that, especially coming from this author, the result is a disappointment.

MARTIN C. NEEDLER

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BOOK NOTES

A book which has had a deservedly prominent place on the desks of a large number of those concerned in one way or another with Indian economic development is *Quiet Crisis in India: Economic Development and American Policy* (Washington, D.C., The Brookings Institution, 1962; xiii, 350 pp., $5.75) by John P. Lewis. It is one of the best introductions available in brief compass to the vast, complex, and often strange entity that is the Indian economy. It strikes an admirable balance between searching criticism, appreciation of achievements, and constructive suggestions that makes it valuable not only to the American

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interested in knowing what is happening to the aid he subscribes to, but even more to the many, Indians and Westerners alike, who are doing their best to bring off a miracle and find it difficult to grasp the overall pattern of which their particular problem is a part. One of Lewis' salient pleas is for greater development of small and medium-sized towns where industrialization can develop without the costly investment in new housing and other services needed when urbanization takes the form of a polarization between the village and the metropolis. It is his hope that India can to some extent by-pass the costly cycle of concentration in large cities followed by a sprawling decentralization that has characterized the development of many Western countries. But while Lewis covers the shortcomings of the Indian administrative system adequately enough for those who are convinced in advance that all governments are inherently inefficient, many who are more sympathetically predisposed toward government activity in general and planning in particular may derive from Lewis' discussion an inadequate impression of the real difficulties in implementing programs for accomplishing this and other objectives. One sometimes gets the feeling that in India when the appropriate order has been issued, the act, as with the decrees of the Mikado, is as good as done, by the self-effectuating power of the order itself, and hence there is no need to take any further implementing steps. Another aspect of this tendency to take the intent for the deed is the reluctance of responsible officials to admit publicly that planning goals will not in fact be met, even when this results in plans for subsequent periods being based on erroneous premises. Lewis also mentions the aversion, rooted in socialist prejudices, to the effective use of the price mechanism as an agency of decentralized control, but the disastrous effects of the attempt to rely on administrative controls over wide areas in the face of an acute shortage of medium-level administrative personnel are only hinted at. In short Lewis' book provides a first rate starting point for an understanding of India's problems; but for an understanding of them in depth more than one book is required, and indeed a lifetime of study may hardly be adequate.

In contrast, the symposium entitled *Administration and Economic Development in India* (Durham, N.C., Duke University Press, 1963; viii, 312 pp., $7.50), edited by Ralph Braibanti and Joseph J. Spengler, is something of a mixed bag. The first two articles by Ralph Braibanti and S. P. Jagota indicate how India came to be endowed with and to maintain a remarkably high level of administrative competence; one is tempted to conclude that the very excellence of this heritage may have seduced the Indians

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into an excessive reliance on administration and direct controls rather than use of market forces. This is balanced by a piece by Wilfrid Malenbaum indicating some of the tasks that will put this leadership to the test. One of these problems is indeed that of Indian agriculture and rural life, which three further articles by Hugh Tinker, Richard L. Park, and Ashok Mitra develop at length. Mitra's paper, indeed, is perhaps the most definite in its conclusions of them all, arguing, in effect, that the wealthier tenth of the agricultural population is greatly undertaxed in comparison with the rest of the population, largely as a result of special privileges granted to agriculture as a whole on the basis of its being a relatively impoverished segment of the economy on the average, and that accordingly this segment of agriculture should be called upon for a greater contribution, through taxation or otherwise, to the development effort. The four remaining articles deal with the institutional environment within which Indian development must take place; S. P. Jagota in a second article deals with the constitutional framework, and the impact on development of caste and other elements of the Hindu tradition are discussed in the remaining three by Robert O. Tilman, Joseph J. Spengler, and N. V. Sovani. While there are interesting insights to be gleaned in this volume, the general impression remains one of a very loosely focused collection.—WILLIAM VICKREY, Columbia University.

Robert C. North, Professor of Political Science at Stanford University, has brought out a new edition of his well-known work *Moscow and Chinese Communists* (Stanford, Calif., Stanford University Press, 1963, 2nd ed.; viii, 310 pp., $7.50). Thirteen chapters are essentially identical with those of the first (1953) edition, but the last two chapters, while incorporating some material from the earlier version, have been rewritten to bring them up to the present date. The chief emphasis remains, as before, on the development of a Chinese mass revolutionary movement in the 1920's through Soviet collaboration with both the "bourgeois" Kuomintang and the fledgling Chinese Communist party, the 1927 break, and its aftermath. In treating the dispute that has developed a generation later between Soviet Russia and its Chinese Communist allies, the author proceeds from the basic assumption that China about 1958 "weighted [national] priorities in favor of economic and technological development at the expense of an early modernizing of its own military establishment" (p. 279), and so began to rely heavily upon the U.S.S.R.'s maintaining a properly militant "anti-imperialist" stance in interna-

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tional affairs. Given the Soviet Union's different strategic approach, the disagreement followed naturally. In the end, Professor North suggests that such basic contemporary factors as population, automation, space exploration, and nuclear energy will probably do more to shape Sino-Soviet relations in the long run than the "day-to-day" problems that have been concerning Chinese and Soviet leaders. Given especially the current revival of some of the Communist doctrinal issues of the 1920's, his history of the development of Sino-Soviet revolutionary relationships from 1917 onward merits thoughtful reading.

A current Harvard University publication, *Communist China 1955–1959: Policy Documents with Analysis* (Cambridge, Mass., Harvard University Press, 1962; xi, 611 pp., $10.00), prepared under the joint auspices of the Center for International Affairs and the East Asian Research Center and carrying a foreword by the respective directors of those two organizations, Robert R. Bowie and John K. Fairbank, admirably supplements the treatment given in the revised section of Professor North's book. It contains forty-eight documents, comprising Chinese policy statements (given in full) on important problems ranging from the speed-up of agricultural cooperativization to ideological reform. The individual items are assembled in chapters according to subject matter, and the work is introduced by a forty-page analysis, correlated to that subject matter, of "Domestic Policy Trends in Communist China, 1955–1959." True, only one document pertains directly to Communist bloc relations. This is but natural: it was only in 1960 that the Sino-Soviet dispute emerged fully into the open. Nevertheless, China's foreign policy is intimately related to domestic developments, and the present volume is a valuable compendium of data basic for an understanding of contemporary China in both its domestic and international aspects. —O. Edmund Clubb, New York City.

The trenchant 1962 James S. Carpentier Lectures—*Law in a Scientific Age* (New York and London, Columbia University Press, 1963; xi, 87 pp., $3.50) by Edwin W. Patterson—deal with relations between contemporary American law and modern science. Professor Patterson's new volume is a suitable philosophic companion for his 1953 *Jurisprudence, Men and Ideas of the Law*. In his latest book he is concerned with the philosophic aspects of three problems: (1) The effects of technological change on the law. (How, for example, the invention of fungicide sprays affected the administration and usefulness of a statute on cedar rust control.) (2) The influence of scientific models on

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legal ideology and legal systematization. (How analogies between scientific procedures and legal methods can curb bias and promote more reliable evaluations in law making processes.) (3) The need for, and progress in, fact research on topics of legal importance (such as, for example, the Gluecks' studies on juvenile delinquency, and the University of Chicago's studies on the functioning of juries). Professor Patterson's analyses are penetrating; he expounds not only the potentialities of a more scientific jurisprudence but also its limitations. His style is clear and graceful; *Law in a Scientific Age* is tightly written but easily read.—CLARENCE MORRIS, University of Pennsylvania Law School.

The *Yearbook of the United Nations, 1961* (New York, Columbia University Press in cooperation with the United Nations, 1963; xii, 813 pp., $16.00), indispensable as a book of reference to all who have serious interest in the work of the United Nations and related agencies, follows the general pattern of the volumes of recent years. It covers the calendar year 1961 and also the proceedings of the General Assembly in January and February 1962. When the General Assembly session extends into the following year, as did the Sixteenth, there is the question of whether the proceedings of the following year should be covered in the *Yearbook* of that year or in the year when the session begins. The editors have chosen the second solution, for which there are obviously strong arguments, but the user, knowing that a matter was discussed and a resolution adopted in the following year, might understandably expect to find what he was after in the *Yearbook* of that year. This, however, is a matter with respect to which the perfect solution is not to be found. The important point to be stressed here is that the editor and those who have collaborated with him are to be congratulated on a job well done under very difficult conditions of limited funds and overworked and changing personnel.—LELAND M. GOODRICH, Columbia University.

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